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HEAT

A MODE OF MOTION.


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HEAT

A MODE OF MOTION.

BY

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PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION OF GREAT BRITAIN.

SIXTH EDITION.

NEW YORK:

D. APPLETON AND COMPANY,

1905.

TO

HIS FRIEND AND TEACHER

ROBERT BUNSEN

THIS BOOK IS DEDICATED

BY

JOHN TYNDALL.

PREFACE

TO

THE SIXTH EDITION.

MY desire and aim in writing and re-editing this book were, and are, to arouse interest as well as to impart instruction. Labour is not to be shirked in the study of a great question; but it may be lightened in two ways: first, by the diminution of its absolute amount; and, secondly, by calling forth an energy which shall diminish it relatively. The true teacher, with the discipline of his pupil in view, will, I apprehend, always invoke the positive in preference to the negative force.

The volume has been out of print for more than two years; for I was unwilling to allow a new edition to appear without such additions and alterations as experience proved to be desirable. The historic development of the subject is more fully dwelt upon in this edition than in previous ones; and here I have to acknowledge my indebtedness to Dr. Gerhard Berthold for his learned memoirs on this and kindred themes. The illustrations of the mechanical production of heat have been varied and multiplied to some extent; new chapters on Electrical

Heat have been introduced, while the sections treating of Chemical and Physiological Heat have been altered and expanded. Throughout the book I have endeavoured to pare away what could be spared, and to add what on reflection seemed worthy of introduction, but which had been previously left out.

Not the results alone of scientific inquiry, but the operations of the inquiring mind, are of interest to the reader here in view. I have therefore tried to show the tendency displayed throughout history, by the most profound investigators, to pass from the world of the senses to a world where vision becomes spiritual, where principles are elaborated, and from which the explorer emerges with conceptions and conclusions, to be approved or rejected according as they coincide, or refuse to coincide, with sensible things. By his observations and reflections in the domain of fact the scientific philosopher is led irresistibly into the domain of theory, his final repose depending on the establishment of absolute harmony between both domains. Thus the motions of the solar system rest securely upon the Principle of Gravitation; light reposes on the Theory of Undulation, while it is the object of this book to show that thermal phenomena find a similar basis in the Mechanical Theory of Heat.

On the continent, science leans on the strong arm of the State; in England its advancement must depend upon the sympathy of the public. Hence the supreme importance, in our case, of spreading abroad correct notions regarding its capacities, achievements, and aims. The practical triumphs of our day are obvious enough, and

they are still frequently spoken of as if they constituted the entire claim of science to the world's attention. To some it seems a kind of handicraft, while others think it is, or ought to be, a mere congeries of facts. But they who regard it thus can know but little of the logic which runs through, and binds together, that 'System of Nature' which it is at once the glory and the responsibility of science to investigate and unfold. Far be it from me to claim for science a position which would exclude other forms of culture. A distinguished friend of mine may count on an ally in the scientific ranks when he Arnold opposes, on behalf of literature, every attempt to render science the intellectual all in all. Ours would be a grey world if illuminated solely by the dry light of the understanding. It needs equally the glow and guidance of high feeling and right thinking in other spheres. But this may be conceded while affirming the just and irrefragable claim of science to a more liberal space in public education than that which it is now permitted to occupy.

J. T.

ROYAL INSTITUTION: *April*, 1880.

The celebrated memoir of Sadi Carnot, entitled 'Réflexions sur la Puissance Motrice du Feu,' first published in 1824, was republished by his brother in 1878; and its reappearance with an appendix of previously unpublished notes demands a reference here. The fundamental principle of this profound essay was, that heat could only pro-

duce work during its passage from a warm body to a cold one. Carnot added the assumption that in the passage no heat was lost. 'La production de la puissance motrice est donc due, dans les machines à vapeur, non à une consommation réelle du calorique, *mais à son transport d'un corps chaud à un corps froid.*' He compares the descent from a high temperature to a low one to the fall of water from a higher to a lower level. 'La puissance motrice d'une chute d'eau dépend de sa hauteur et de la qualité du liquide; la puissance motrice de la chaleur dépend aussi de la qualité de calorique employé et de ce qu'on pourrait nommer, de ce que nous appellerons en effet, la *hauteur de sa chute*, c'est-à-dire de la différence de température des corps entre lesquels se fait l'échange du calorique.' Clausius was the first to disengage from the reasonings of Carnot the erroneous assumption that in the production of motive power no heat was lost. But from the unpublished notes of Carnot himself above referred to, it appears that, before his death in 1830, he had clearly disentangled his mind from his first assumption.

In these notes he combats the notion of the materiality of heat, referring to the experiments of Rumford to prove that it is a kind of motion. 'Ainsi,' he says, 'la chaleur est créée par le mouvement. Si elle est une matière, il faut admettre que la matière est créée par le mouvement.' In his day the undulatory theory of light had already taken root in men's minds, and Carnot, in those inedited notes, connects that theory with the dynamical theory of heat. 'On regarde aujourd'hui généralement la lumière comme le résultat d'un mouvement de vibration du fluide

éthéré. La lumière produit de la chaleur, ou, au moins, elle accompagne la chaleur rayonnante, et se meut avec la même vitesse qu'elle. La chaleur rayonnante est donc un mouvement de vibration. Il serait ridicule de supposer que c'est une émission de corps, tandis que la lumière qui l'accompagne ne serait qu'un mouvement. Un mouvement pourrait-il produire un corps? Non, sans doute, il ne peut produire qu'un mouvement. La chaleur est donc le résultat d'un mouvement. Alors il est tout simple qu'elle puisse se produire par la consommation de puissance motrice, et qu'elle puisse produire cette puissance.'

The notes are replete with clear and pregnant remarks, one or two of the most remarkable of which may be here reproduced. 'La chaleur n'est autre chose que la puissance motrice, ou plutôt que le mouvement qui a changé de forme. C'est un mouvement dans les particules des corps. Partout où il y a destruction de puissance motrice, il y a, en même temps, production de chaleur en quantité précisément proportionelle à la quantité de puissance motrice détruite. Réciproquement, partout où il y a destruction de chaleur, il y a production de puissance motrice.'

'On peut donc,' he continues, 'poser en thèse générale que la puissance motrice est en quantité invariable dans la nature, qu'elle n'est jamais, à proprement parler, ni produite ni détruite. A la vérité, elle change de forme, c'est-à-dire qu'elle produit tantôt un genre de mouvement, tantôt un autre; mais elle n'est jamais anéantie.'

Carnot appears to have clearly realised the idea of a strict numerical equivalence between heat and motive

power. 'D'après quelques idées que je me suis formées sur la théorie de la chaleur, la production d'une unité de puissance motrice nécessite la destruction de 2·70 unités de chaleur.' Assuming with Carnot's brother the unit of motive power here referred to, to be the raising of a cubic metre of water, one metre high, Carnot's evaluation would make the mechanical equivalent of heat 370 kilogram-metres. What the basis of this evaluation was does not appear, but it does no violence to probability to suppose that a mind so penetrating had made use of the ratio of specific heat at constant pressure to specific heat at constant volume afterwards employed by Mayer. The point of highest historic interest here is that no great step in science is made in an isolated manner. The spirit of the age, if we may use a vague term, is more or less impregnated with the new conception before it receives distinct enunciation.

Carnot proposed to himself the following experiments, which show how completely he had grasped the true relation between heat and work. 'Répéter l'expérience de Rumford sur le forage d'un métal dans l'eau, mais mesurer la puissance motrice consommée en même temps que la chaleur produite; mêmes expériences sur plusieurs métaux et sur le bois.' The use of the word 'mais' in this passage indicates that Carnot proposed, not only to repeat the experiment of Rumford, but to supplement it in the direction which gives the experiment its chief scientific significance. He proceeds:—'Frapper un morceau de plomb en plusieurs sens, mesurer la puissance motrice consommée et la chaleur produite. Mêmes expériences sur d'autres métaux.' Again:—'Agiter fortement de l'eau dans un barillet ou

dans un corps de pompe à double effet et dont le piston serait percé d'une petite ouverture.' Again:—'Expériences du même genre sur l'agitation du mercure, de l'alcool, de l'air et d'autres gaz. Mesurer la puissance consommée et la chaleur produite.' Carnot also refers to the expansion of gases as furnishing a means of comparing the heat consumed with the work produced, and *vice versa.*

With reference to the law of equivalence Monsieur H. Carnot, the brother and editor of Sadi Carnot, makes the following just remarks:—'Ces lois, la loi d'équivalence du moins, était ignorée de tous, et de Sadi Carnot lui-même, lorsqu'il composa son livre. Elle se dégagea peu à peu dans la suite de ses travaux. Il arriva à la concevoir et à la formuler exactement: ses notes manuscrites, ses programmes d'expériences ne laissent aucun doute à cet égard. On sera frappé, en les lisant, de l'analogie qui existe entre certaines des idées qu'il exprime et celles qui ont été plus tard développées par Mayer, entre ses projets d'expériences et les expériences qui ont été réalisées par Joule. Il est bien entendu que la similitude dont nous parlons ne diminue en rien le mérite de ces savants, puisqu'ils n'eurent pas connaissance des travaux de leur prédécesseur.'

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HEAT

A MODE OF MOTION.

LECTURE I.

INTRODUCTION—INSTRUMENTS EMPLOYED—GENERATION OF HEAT BY FRICTION, COMPRESSION, AND PERCUSSION—MODES OF OBTAINING FIRE EMPLOYED BY SAVAGES—MECHANICAL HEATING AND CHILLING OF AIR—FORMATION OF CLOUDS—SOURCE OF POWER IN THE THERMO-ELECTRIC PILE.

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THE aspects of Nature provoke in man the spirit of inquiry. As the eye is formed to see, and the ear to hear, so the human mind is formed to explore and understand the basis and relationship of natural phenomena. A modern discovery illustrates the manner in which our present mastery over nature has been obtained. We start with a magnet of infinitesimal power, which gives rise to electric currents of infinitesimal strength. These react upon the magnet, exalt its attractive and repulsive forces, thus enabling it to produce stronger currents, which again react upon and enhance the power of their source. Thus we rise from an origin too feeble to produce the slightest spark or gleam, to an energy competent to produce the solar brilliancy of the electric light. In a similarly small way the human mind began its operations among the powers of Nature; winning

first a little knowledge and a little strength, and then turning the knowledge and the strength so won back upon Nature, with the view of winning more. Action and reaction have thus gone on from prehistoric ages to the present time. The result is that stored body of scientific knowledge, and that developed power of scientific investigation, which have revolutionised philosophy, and begotten those marvels of practical Science in the midst of which we dwell.

Seventeen years ago, after some tentative trials in previous years, I endeavoured to bring one of the most important results of this interaction of Man and Nature before the members of the Royal Institution, and subsequently, in a more permanent form, before the world at large. After much study of the subject, and much reflection on the mode of treating it, I ventured to expound in familiar language and by simple experiments, the grounds and outcome of the momentous doctrine known as the Conservation of Energy, or the Conservation of Force. To-day we re-enter this great domain, with the view of reviving our knowledge and extending its range.

Whether we regard its achievements in the past, or its promise in the future, the whole tendency of Physical Science is to confirm the dictum of the poet that

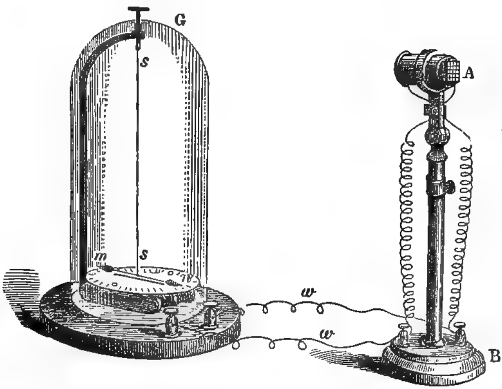
> All are but parts of one stupendous whole.

And the discipline and delight of our present work will consist, not in the registration of independent phenomena, but in the discovery of bonds and connexions which show the various parts and powers of Nature to be as definitely related to each other as are the organs and processes of the living body itself.

It is my first duty to make you acquainted with some of the instruments intended to be employed in the ex-

amination of the doctrine just referred to. Some means must be devised of making the indications of heat and cold visible to you, and for this purpose an ordinary thermometer would be useless. You could not observe its action; and as it is my desire that we should be copartners in these labours, I am anxious that you should see with your own eyes the facts on which our subsequent philosophy is to be based. To secure this end, I have been obliged to abandon the use of a common thermometer, and to resort to the little instrument now to be described.

FIG. 1.



This instrument, A B (fig. 1), is called a *thermo-electric pile*, or more briefly a *thermo-pile*. It acts thus:—When heat is communicated to the face A of the pile it generates an electric current; and an electric current has the power of deflecting a freely suspended magnetic needle, to which it flows parallel. Before you is placed such a needle, *m n* (fig. 1), in part surrounded by a covered copper wire, the free ends of which, *w w*, are connected with the thermo-pile. The needle is suspended by a fibre, *s s*, of cocoon silk, and protected by a glass shade, G, from all disturbance by currents of air. To one end of the needle is fixed a piece

of red and to the other end a piece of blue paper. All of you see these pieces of paper, and when the needle moves its motion will be clearly visible to the most distant person in this room.

This instrument is called a *galvanometer*.[1]

At present the needle is quite at rest, and points to the zero-mark on the graduated disk underneath it. This shows that there is no current passing. I breathe for an instant against the naked face A of the pile—a single puff of breath is sufficient for my purpose—the needle starts off and passes through an arc of 90°. It would go farther did we not limit its swing by fixing, edgeways, a thin plate of mica at this point. This action is produced by the small amount of warmth communicated by my breath to the face of the pile, and no ordinary thermometer could give so large and prompt an indication. Take notice of the direction of the deflection; the red end of the needle moved from me towards you. We will let the heat waste itself; it does so rapidly, and as the pile cools, the needle returns to its first position. Chilling a plate of metal by placing it on ice, I wipe the metal, and touch with it the face of the pile. A moment's contact suffices to produce a prompt and energetic deflection of the needle. But mark the direction of the deflection. When the pile was warmed, the red end of the needle moved from me towards you; the same end now moves from you towards me. The important point here established is, that from the direction in which the needle moves we can, with certainty, infer whether cold or heat has been communicated to the pile; and the energy with which the needle moves—the promptness with which it is driven aside from its position

[1] In the actual arrangement the galvanometer here described stood on a stool in front of the lecture table, the wires *w w* being sufficiently long to reach from the table to the stool. For a further description of the instrument see the Appendix to this Lecture.

of rest—gives us some idea of the comparative quantities of heat or cold imparted in different cases. On a future occasion we may learn how to express with numerical accuracy the relative quantities of heat communicated to the pile, for the present a general knowledge of our instrument is sufficient.

MECHANICAL HEAT.

My desire now is to illustrate with sufficient fullness the mechanical generation of heat. From the next room, which is cooler than this one, my assistant brings a piece of wood which ought to be slightly colder than the pile. The face of the instrument being placed against the piece of wood, the red end of the needle moves from you towards me, thus showing that the contact has chilled the instrument. I now very gently and very carefully rub the face of the pile along the surface of the wood—'carefully,' because the pile is brittle, and rough usage would destroy it. The prompt motion of the red end of the needle towards you declares that the face of the pile has been heated by this small amount of friction. A flat brass button, attached to the end of a cork, which, when taken hold of, preserves the brass from the warmth of the hand, is placed against the face of the pile. The needle moves, showing that the metal is cold. A moment's rubbing on the surface of a cold piece of wood renders the brass so hot, that if allowed to remain long in contact with the pile the current generated would dash the needle violently against its stops, and probably derange its magnetism. An instant's contact produces a strong deflection. I rub a razor, which has been cooled by contact with ice, along a dry hone, as if to sharpen it. On placing the razor against the face of the pile, the steel, which a moment ago was cold, is declared hot. Along a cold knife-board, I rub a cold knife. Placed afterwards against the pile,

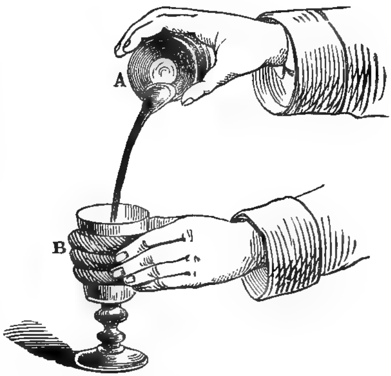
the knife declares itself to be hot. I pass a cold saw through a cold piece of wood, and bring the rubbed surface of the wood into contact with the pile. The needle instantly moves in a direction which shows the wood to be heated. Allowing the needle to return to zero, and applying the saw itself to the pile, it also is proved hot. These belong to the simplest and most commonplace examples of the generation of heat by friction, and they are chosen for this reason. Humble as they appear, they are illustrations of a principle which determines the polity of the whole material universe.

Heat is also produced by compression. Placing a lead bullet between the plates of a small hydraulic press, I squeeze it to flatness. Brought into contact with the pile, the galvanometer declares that heat has been developed by the compression. Percussion produces a similar effect. I place a cold lead bullet upon a cold anvil, and strike it with a cold sledge-hammer. The motion of the hammer is suddenly arrested; apparently the force with which the sledge descends is destroyed. But when we examine the flattened lead we find it heated; and we shall by-and-by learn that, if we could gather up all the heat generated by the shock of the sledge, and apply it without loss mechanically, we should be able, by means of it, to lift the hammer to the height from which it fell.

Another experiment is here arranged, which is almost too delicate to be performed with our large apparatus, but which, nevertheless, is easily executed with proper instruments. This small basin contains a quantity of mercury which has been cooled in the next room. One of the faces of the thermo-electric pile being coated with a protective varnish, is plunged into the liquid metal. The deflection of the needle proves that the mercury is cold. Two glasses, A and B (fig. 2), are swathed thickly round with listing, to prevent the warmth of the hands from

reaching the mercury. I pour the cold mercury into one of the glasses, and then from the one glass into the other, and back. Its motion is destroyed, but heat is developed. The amount of heat generated by a single pouring out is extremely small; so we will repeat the process ten or fifteen times. The pile being now plunged into the liquid, the needle moves; and its motion declares that the mercury, which at the beginning of the experiment was cooler, is now warmer than the pile. We here introduce into the lecture-room an effect which occurs at the base of every waterfall. There are friends before me who have stood amid the foam of Niagara, and I have done so myself. Had we dipped sufficiently sensitive thermometers into the water at the top and at the bottom of the cataract, we should have found the latter warmer than the former. The sailor's tradition, also, is theoretically correct; the sea is rendered warmer by a storm, the mechanical dash of its billows being ultimately converted into heat.[1]

FIG. 2.



[1] I say 'theoretically correct, because it would require far more care and instrumental delicacy than appear to have been invoked, to prove that the observed differences of temperature between sea and air were due solely to mechanical action. Nevertheless the tradition is an old one, as the following quotation proves:

'In one of those gales on September 12, Dr. Irving tried the temperature of the sea in that state of agitation, and found it considerably warmer than that of the atmosphere. This observation is the more interesting, as it agrees with a passage in Plutarch's Natural Questions, not

Whenever friction is overcome, heat is produced, and the heat produced is the exact measure of the power expended in overcoming the friction. The heat is simply the original power in another form, and if we wish to postpone this conversion, we must abolish the friction. We place oil upon the surface of a hone, we grease a saw, and we are careful to lubricate the axles of our railway carriages. What is the real meaning of these acts? It is the object of a railway engineer to urge his train from one place to another; and it is not his interest to allow any portion of his force to be applied in a manner which would not promote the attainment of his object. He does not want his axles heated, and hence he avoids as much as possible expending his power in heating them. He has obtained his force from heat, and it is not his object to reconvert by friction the force thus obtained into its primitive form. For every degree of temperature generated in his axles, a definite amount would be withdrawn from his urging force. There would be no absolute loss. Could he gather up all the heat generated by the friction, and apply it mechanically, he would, by it, be able to impart to the train the precise amount of speed which it had lost by the friction. Every one of those railway porters whom you see moving about with his can of yellow grease, and opening the little boxes which surround the carriage axles, is, without knowing it, illustrating a principle which forms the very solder of Nature. In the long run, however, the generation of heat cannot be avoided. All the force of our locomotives eventually takes this form. To maintain the proper speed, the friction of the train must be continually overcome, and the

I believe, before taken notice of, or confirmed by experiment, in which he remarks that the sea becomes warmer by being agitated in waves.'—'A Voyage to the North Pole, undertaken by his Majesty's commands 1773, by Constantine John Phipps.'

force spent in overcoming it is entirely converted into heat. An eminent writer[1] has compared the process to one of distillation: the heat of the furnace distils into the mechanical motion of the train, and this motion recondenses as heat in the wheels, axles, and rails.

So also with regard to the greasing of a saw by a carpenter. He applies his force with the express object of cutting through the wood. He wishes to overcome mechanical cohesion by the teeth of his saw, and when it moves stiffly, the same amount of effort may produce a much smaller effect than when the implement moves without friction. But in what sense smaller? Not absolutely so, but smaller as regards the act of sawing. The force not expended in sawing is misapplied, not lost; it is converted into heat. Here again, if we could collect the heat engendered by the friction, and apply it to the urging of the saw, we should make good the precise amount of work which the carpenter, by neglecting the lubrication of his implement, had simply converted into another form of power.

We warm our hands by rubbing, and in cases of frostbite we thus restore animation to the injured parts. By friction a lucifer-match is raised to the temperature of ignition. In the common flint and steel the particles of the metal struck off are so much heated by the collision that they take fire and burn in the air. But the heat precedes the combustion. Hooke proved this; and Davy found that when a gunlock with a flint was discharged in vacuo, no sparks were produced, but the particles of steel struck off, when examined under the microscope, showed signs of fusion.[2] Before the safety-lamp was invented the workers in our coal-mines derived their light from showers of sparks, generated by the friction

[1] Robert Julius Mayer, lately deceased.

[2] Works of Sir H. Davy, vol. ii. p. 8.

of flint against the edge of a swiftly rotating steel wheel, the sparks having been considered incompetent to ignite the 'fire-damp.' Aristotle refers to the heating of arrows by the friction of the air; and the most probable theory of shooting stars is that they are small planetary bodies revolving round the sun, which being caused to swerve from their orbits by the attraction of the earth, are raised to incandescence by friction against our atmosphere. Chladni propounded this view and Dr. Joule has confirmed it by calculation. He may, moreover, be correct in believing that the earth is spared bombardment through the breaking up of our aërolites by heat.[1] These bodies move at planetary rates. The orbital velocities of the four Interior planets are as follows:

	Miles per Second.
Mercury	30·40
Venus	22·24
Earth	18·91
Mars	15·32

while the velocity of the aërolites varies from 18 to 36 miles a second. The friction engendered by this enormous speed is no doubt competent to produce the effects ascribed to it.

Knowing the velocity and weight of any projectile, we shall subsequently learn how to calculate the amount of heat developed by the destruction of its motion. For example, knowing as we do the weight of the earth and the velocity with which it moves through space, a simple calculation enables us to state the exact amount of heat which would be developed, supposing the earth to strike against a target strong enough to stop its motion. We could tell, for example, the number of degrees which this amount of heat would impart to a globe of water equal to the earth in size. Mayer,

[1] 'Philosophical Magazine,' 4th Series, vol. xxxii. p. 349.

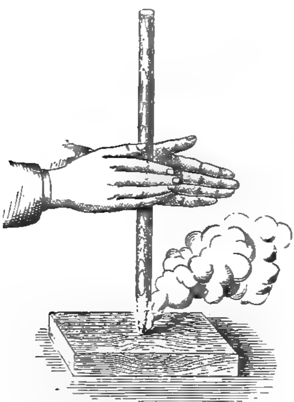
Helmholtz, and Thomson have made this calculation, and found that the quantity of heat corresponding to this colossal shock would be quite sufficient, not only to fuse the entire earth, but to reduce it, in great part, to vapour. Thus, by the simple stoppage of our planet in its orbit, 'the elements' might be caused 'to melt with fervent heat.' The amount of heat thus developed would be equal to that derived from the combustion of fourteen globes of coal, each equal to the earth in magnitude. And if, after the stoppage of its orbital motion, the earth should fall into the sun, as it assuredly would, the amount of heat generated by the blow would be equal to that developed by the combustion of 5,600 worlds of solid carbon. Knowledge such as that which you now possess has caused philosophers, in speculating on the mode in which the sun's power is maintained, to suppose solar heat and light to be caused by the showering down of meteoric matter upon the sun's surface. To this subject we shall return in due time.

The life of the human race may be divided into two great periods, the prehistoric and historic. But human beings had done great things before they learnt to write about their doings. Among other things, they had discovered the use of fire, both as a means of warming their bodies and cooking their food. Nobody can tell how or when fire was first introduced. Lucretius has a story which ascribes its origin to the rubbing together of dry tree branches; but this is not a likely source of ignition. Forests are sometimes set ablaze by lightning, and this is a possible origin of our domestic fires. Again, savages have everywhere employed stone implements, shaping pieces of flint with sharp edges for knives, and with sharp points for arrow-heads and spears. Sparks were certainly thus produced, and such sparks may have been the ancestors of our fires. At the present

hour the inhabitants of Tierra del Fuego employ two stones, the one a hard flinty pebble, the other a lump of iron pyrites. The stones are before you, and when they are thus brought into sharp collision, heated particles are struck off from the pyrites, which combine with the atmospheric oxygen and hang as red sparks for a sensible interval in the air.

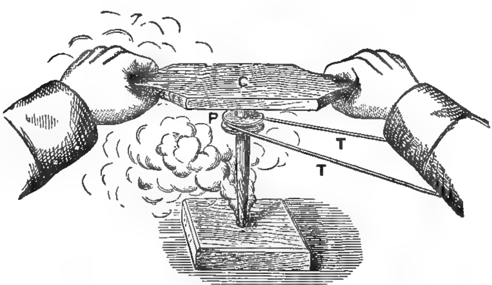
Friction, however, is the skilful savage's ordinary resource. Before you is a slab of dry oak, with grooves charred in it by the rapid passage to and fro of a pointed stick of mahogany. This is what Mr. Tylor calls 'the stick and groove machine.' It is easy to produce smoke with it, but it is not easy to produce fire. Mr. Darwin tried his hand on it at Tahiti. 'The fire,' he says, 'was produced in a few seconds; but to a person who does not understand the art, it requires the greatest exertion, as I found, before at last, to my great pride, I succeeded in igniting the dust.' The Gaucho of the Pampas presses the blunt end of an elastic stick about eighteen inches long against his breast, and the other end, which is pointed, into a hole in a piece of wood. Bending the stick by the pressure of his body, he seizes the curved part, turns it rapidly round, and thus produces fire. In Australia and Tasmania fire is evoked as shown in fig. 3 by the rapid twirling of a pointed stick between the palms. In the case of the Esquimaux, one person presses the end of a stick against a piece of wood, while a second, by means of a thong, produces a rapid to-and-fro rotation, thereby evoking fire.

FIG. 3.



With the band T, T (fig. 4), from a whirling table, instead of a thong held in the hand, I will endeavour to show you this production of heat. Pressing by means of a piece of wood C, a pin, P of beech, mulberry, or some other wood, into a hollow formed in a slab of oak or mahogany, and causing the pin to rotate rapidly, in a few seconds dense and stifling fumes rise from the place of friction. Both the rotating pin and the hollow in which it revolves are charred; and so rapid is this effect, that a pin three or four inches long is wholly reduced in a few minutes to charcoal powder. It is not easy to produce actual ignition in fact, with the woods hitherto employed I have failed to

FIG. 4.



produce it. But brushing with a feather a small quantity of fulminating powder against the rotating pin, it mixes with the charcoal powder and gently ignites, the wood continuing to blaze afterwards as long as the rotation continues.

MECHANICAL HEATING AND CHILLING OF AIR. FORMATION OF CLOUDS.

One of the most interesting illustrations of the development of heat by mechanical means is that of the fire syringe. The instrument which I here bring before you

consists of a strong cylinder of glass T U (fig. 5), accurately bored, and quite smooth within. Into it a piston fits air-tight, so that by driving the piston down, the air under-

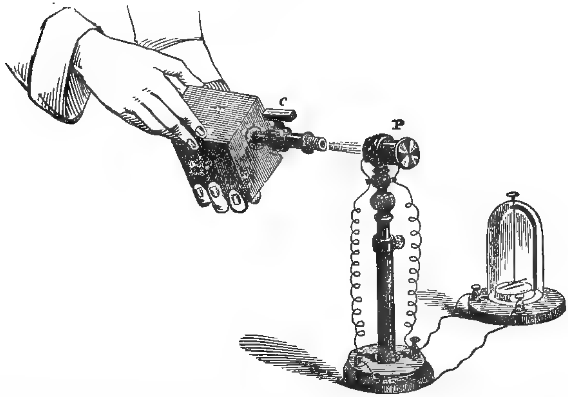
FIG. 5.

neath is forcibly compressed; and when the air is thus compressed, heat is suddenly generated. Tinder, or dry amadou, may be ignited by this syringe, but we will adopt another method of rendering the heat visible. I wet a pellet of cotton-wool with liquid bisulphide of carbon, throw the bit of wet cotton into the glass syringe, and instantly eject it. It has left behind a residue of vapour. On compressing the air suddenly, the heat developed is sufficient to ignite the vapour, and produce a flash of light within the syringe. Placing the cotton again in the tube, I urge the piston downwards; you see the flash as before. Blowing away in each case the fumes generated by the combustion of the vapour and permitting the cotton to remain in the syringe, the experiment may be repeated and the flash of light obtained twenty times in succession.

In all the cases hitherto introduced to your notice, heat has been *generated* by the expenditure of mechanical force. I wish now to bring before you the converse effect, and show you the *consumption* of heat in mechanical work. This strong iron vessel (v, fig. 6) is filled at the present moment with compressed air. It has lain here for some hours, so that the temperature of the air within the vessel is now the same as that of the air of the room. At the present moment this inner air is pressing against the sides of the vessel, and if this cock *c* be opened, a portion of the air will rush violently out. The word 'rush,' how-

ever, but vaguely expresses the true state of things; the air which issues is driven out by the air behind it;

FIG. 6.



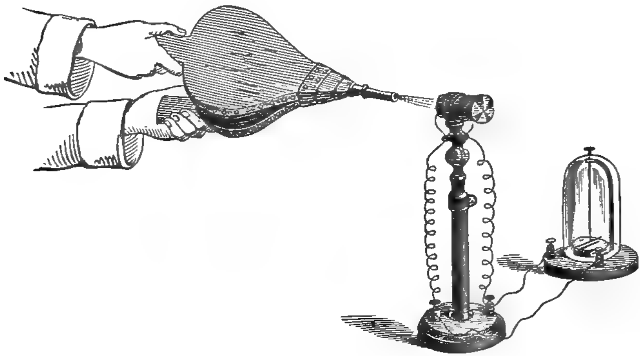
this latter accomplishes the work of urging forward the stream of air. And what will be the condition of the *working air* during this process? It will be chilled. In fact, the only agent it can call upon to perform the work is the heat to which its elastic force is due. A portion of this heat will be consumed, and a lowering of temperature will be the consequence. I turn the cock and allow the air from the vessel to strike against the face of the pile P. The needle instantly responds; its red end is driven towards me, thus declaring that the pile has been *chilled* by the current of air.

But further instruction may be derived from this form of experiment. Our iron box is seven inches long, with a cross section of five inches square. I charge it with fifty strokes of a compressing syringe. The air within the box, and to some extent the box itself, are now warm. On turning the cock *c* rapidly quite round, a puff escapes which sends the needle in the direction of heat. This may be done three or four times in succession. But the heating

at length ceases, and finally the needle moves in the direction of cold. The reason is plain. This last air has done most work, and has thus used up its warmth: hence its power to chill the pile.

In the case of a common pair of bellows (fig. 7), when the boards are separated, the air rushes in. When

FIG. 7.

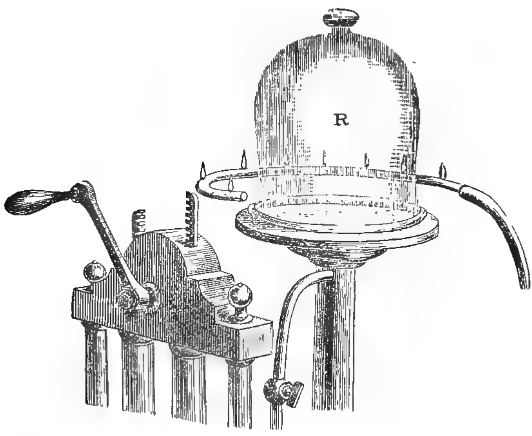


they are pressed together, the air rushes out. The expelled air, slightly warmed by compression, immediately striking the face of the pile, produces the deflection due to heat. But if the nozzle be so far withdrawn as to leave the compressed air time to expand, a chilling instead of a warming of the pile is the consequence.

One important effect connected with this chilling of air by its own rarefaction is now to be noted. On the plate of the air-pump is placed a large glass receiver, filled with the air of this room. This air, and, indeed, all air, unless it be dried artificially, contains a quantity of aqueous vapour, which, as vapour, is perfectly invisible. A certain temperature is requisite to maintain the vapour in this invisible state; and if the air be chilled so as to bring it below this temperature, the vapour will instantly condense,

and form a visible cloud. Such a cloud, which you will remember is not *vapour*, but *liquid water* in a state of fine division, will form within this glass vessel R (fig. 8), when the air is pumped out of it; and to make the effect visible to those right and left of me, as well as to those in front, eight little gas jets are arranged in a semicircle which half surrounds the receiver. When the cloud forms, the dimness which it produces will at once declare its presence. A very few strokes of the pump suffice to precipitate the vapour. It spreads throughout the entire

FIG. 8.



receiver, and many of you see a colouring of the cloud, as the light shines through it, similar to that observed sometimes, on a large scale, around the moon. When the air is allowed to re-enter the vessel, it is heated; the cloud melts away, and the perfect transparency of the air within the receiver is restored. Pour half the wine from a freshly opened bottle of champagne; recork the bottle, and permit it to rest till the pressure of the carbonic acid is restored. On suddenly reopening the

bottle the expansion of the gas produces a dense aqueous cloud in the previously clear space above the liquid.

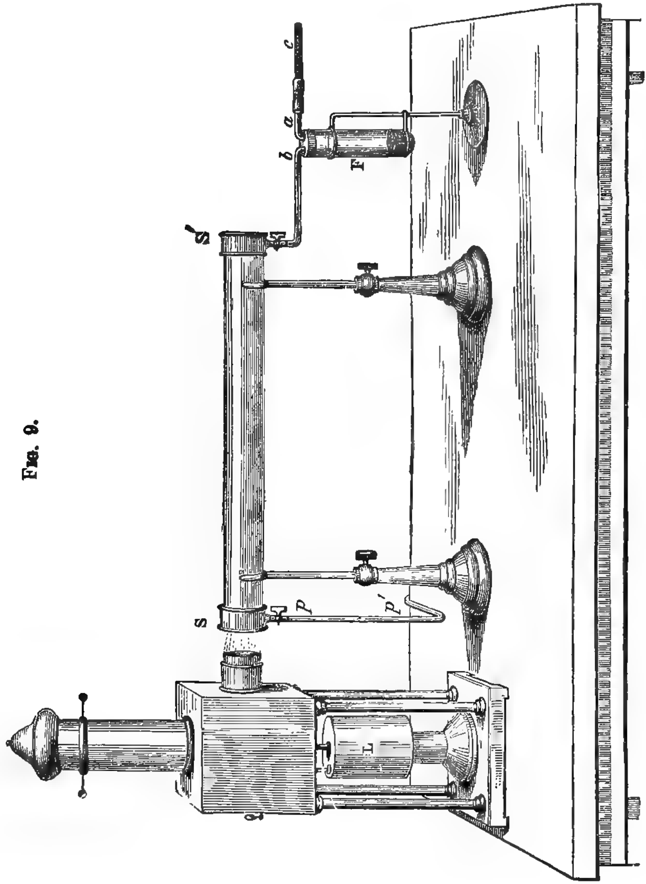
Clouds may be produced by the precipitation of various vapours other than that of water. To prove this in your presence, it is only necessary to exhaust a glass tube three feet long and three inches wide, and to refill it with air which has been permitted to bubble through the liquid whose vapour is to be experimented on. By means of a stopcock the filled tube may be suddenly connected with the exhausted cylinder of an air-pump. The air is driven into the cylinder, the tube which contains the working air becoming immediately filled with a cloud through the precipitation of the chilled vapour. The whole arrangement is shown in fig. 9, where L represents an electric lamp which sends a condensed luminous beam into the glass tube S S′, which is about three inches wide. F is a flask containing the liquid through which the air passes, entering the flask by the tube *a*, and escaping, vapour-laden, by the tube *b* into the larger tube S S′. The open end of the tube *a* is stopped with a plug of cotton-wool *c* to hold back the floating dust of the air.

The tube S S′ is first exhausted by an air-pump connected with the pipe *p p′*. The cock *p* being shut, the tube S S′ is filled with vapour-laden air. The stopcock to the right is then turned off, and the cylinders of the air-pump are exhausted. On opening the cock *p* the air rushes into the cylinders, the precipitation of a cloud in the tube being the immediate consequence. Allowing the cloud to melt away, and shutting off the residue of air and vapour after each precipitation, it is possible by exhausting the cylinders, and repeating the process, to obtain, with some substances, fifteen or twenty clouds in succession from a single charge of vapour-laden air.

The clouds thus precipitated differ from each other in luminous energy, some shedding a mild white light, while

others flash forth with surprising suddenness and brilliancy. This is due to the fact, that the liquids from which the clouds are formed differ in their powers of

Fig. 9.



refraction and reflection. Different clouds, moreover, possess very different degrees of stability; some melt rapidly away, while others linger for minutes in the tube, resting

finally upon its bottom, and disappearing like a dissolving heap of snow. Nothing can exceed the splendour of the iridescences exhibited by many of these clouds.

Let us look a little more closely at this process of cloud formation. The moment before precipitation occurs the whole mass of cooling air and vapour may be regarded as divided into a number of small polyhedra, each of which is subsequently applied to the formation of a single particle of cloud. It is manifest that the size of each particle must depend not only on the size of its vapour polyhedron, but also on the density of the vapour as compared to that of its liquid. Other things being equal, if the vapour be light and the liquid heavy, the cloud-particle will be smaller than if the vapour be heavy and the liquid light. Let us compare in this respect toluol and water. The specific gravity of the former liquid is 0·85, that of water being 1. But the specific gravity of toluol vapour is 3·26, that of aqueous vapour being only 0·6. Here, therefore, we have a light liquid with a heavy vapour to compare with a heavier liquid and a light vapour. The consequence is, that the cloud of toluol is far coarser than that of water. To the properties of water and its vapour here referred to, the soft and tender beauty of the clouds of our atmosphere is probably in great part to be ascribed.

Sir Humphry Davy refers in his 'Chemical Philosophy' to a machine at Schemnitz, in Hungary, in which air was compressed by a column of water 260 feet in height. When a stopcock was opened so as to allow the air to escape, a degree of cold was produced which not only precipitated the aqueous vapour diffused in the air, but caused it to congeal in a shower of snow, while the pipe from which the air issued became bearded with icicles. 'Dr. Darwin,'[1] writes Davy, 'has ingeniously explained

[1] Grandfather of Mr. Charles Darwin.

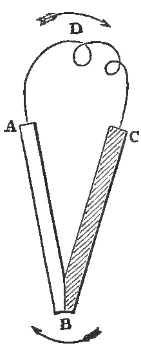
the production of snow on the tops of the highest mountains, by the precipitation of vapour from the rarefied air which ascends from plains and valleys. The Andes, placed almost under the line, rise in the midst of burning sands; about the middle height is a pleasant and mild climate; the summits are covered with unchanging snows.'

SOURCE OF POWER IN THE THERMO-ELECTRIC PILE.

And now if you grant me your patience, we will investigate a subtler example of the transformation and restitution of heat than any hitherto brought before you, which will open up to us, moreover, the philosophy of the thermo-electric pile. A few words on the construction of the instrument will be useful at the outset.

Let A B (fig. 10) be a bar of antimony, and B C a bar of bismuth, and let both bars be soldered together at B. Let the free ends A and C be united by a piece of wire, A D C. On warming the place of junction, B, an electric current is generated, the direction of which is from bismuth to antimony, across the junction, and from antimony to bismuth, through the connecting wire, A D C. The arrows indicate the direction of the current.

FIG. 10.

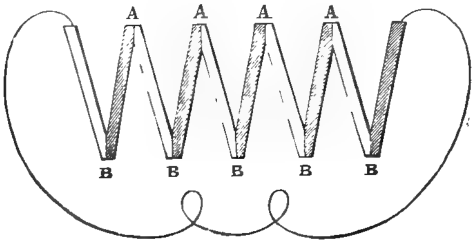


If the junction B be *chilled*, a current is generated opposed in direction to the former. The figure represents what is called a thermo-electric pair or couple.

By the union of several thermo-electric pairs a more powerful current can be generated than could be obtained from a single pair. Fig. 11, for example, represents such an arrangement, in which the shaded bars are supposed to be bismuth, and the unshaded ones antimony; on warming all the junctions, B, B, &c., a

current is generated at each, and the sum of these currents, which all flow in the same direction, will produce a stronger resultant current than that obtained from a single pair.

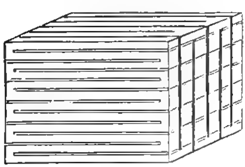
FIG. 11.



The V formed by each pair need not be so wide as it is shown in fig. 11; it may be contracted without prejudice to the couple. And if it is desired to pack several pairs into a small compass, each separate couple may be arranged as in fig. 12, where the black lines represent small bismuth bars, and the white ones small bars of antimony. They are soldered together at the ends, and throughout their length are usually separated, not by the wide spaces shown in fig. 12, but by strips of paper merely. A collection of pairs thus compactly set together constitutes a thermo-pile, a drawing of which is given in fig. 13.

FIG 12.

FIG. 13.



The current produced by heat being always from bismuth to antimony, across the heated junction, a moment's inspection of fig. 11 will show that when any one of the junctions, A, A, is heated, a current is generated opposed in direction to that generated when the heat is applied to the junctions B, B. Hence in the case of the thermo-pile, the effect of heat falling upon its two opposite faces is

to produce currents in opposite directions. If the temperature of the two faces be alike, they neutralise each other, no matter how highly they may be heated absolutely; but if one of them be warmer than the other, a current is produced. The current is thus due to a *difference* of temperature between the two faces of the pile, and within certain limits the strength of the current is exactly proportional to this difference.

Let us now consider the observed performance of this little instrument. When the face of the pile was heated, our heavy needle started briskly aside, reached a deflection of 90°, struck against its stops, the visible motion then destroyed being converted into heat. Had a thermometer of sufficient delicacy been brought into contact with the needle and the stop, both would have been found warmed by the concussion. Those that were near enough may have heard the tap of the needle against the mica, the power of that subtle something which I have called an electric current being thus, in part, converted into sound. By the mutual friction of the particles among which it passed the tiny sound-pulse was rapidly converted into heat. The further oscillations of the needle were soon exhausted by the friction of the air and of the suspending fibre, this friction also producing its own modicum of heat. If, finally, we had examined the wire of the galvanometer we should have found it also warm. In short, both the electric current itself and all the mechanical motions it produces take ultimately the form of heat.

Now the chief burthen of these lectures is to show that to do work we must expend power; the power so expended being the exact equivalent of the work done. What then is the source of the power which can thus directly, by its own action, or indirectly, by mechanical motion and sound, generate heat? You doubtless would be disposed, and rightly disposed, to seek the origin of this distant

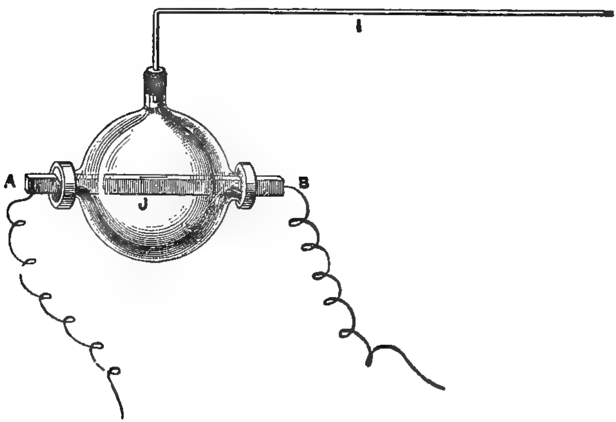
manifestation of power in the heat originally communicated to the pile. But how does it act? Neither you nor I can completely answer this question, for nobody fully knows what occurs at the warmed face of the pile. Our knowledge, however, enables us to see that we have here no infraction of the principle, that to produce the motion of the needle, the sound of its concussion, and the warmth of the coil, an equivalent amount of power has been expended at the proper place.

Thermo-electricity was discovered in 1826 by Thomas Seebeck of Berlin. The first thermo-pile was made by the celebrated Italian Nobili, and in the hands of his equally celebrated countryman Melloni it became an instrument so important as to supersede all others in researches on Radiant Heat. To this purpose it will be applied in future lectures; our present object is to ascertain the source of its power. Here an observation made by Peltier in 1834 comes to our aid. Soldering the end of a bar of bismuth to the end of a bar of antimony, and connecting the other two ends with a weak voltaic battery, Peltier sent feeble currents, first from antimony to bismuth, and then from bismuth to antimony, across the soldered junction. In the first case he found the junction always warmed, and in the second case always chilled by the current. An actual consumption of heat was thus shown to take place, when a voltaic current of sufficiently low intensity was sent from bismuth to antimony across a junction of the two metals.

I wish now to prove to you by actual experiment that heat and cold are produced in accordance with the statement of Peltier. This glass bulb (fig. 14) has three tubulures, through two of which pass air-tight the bar of bismuth B and the bar of antimony A, both bars being soldered together at J. A narrow glass tube passes air-tight through a cork stopping the third tubulure. A little

above the cork the tube is bent at a right angle, and in its horizontal portion rests a short column of coloured liquid I, which is to serve as our index. Placing this portion of the narrow tube in the beam of an electric lamp, a magnified image of the index and the adjacent tube is cast upon a screen. Through the wires connected with A and B we now send a current from a single voltaic cell, causing it first to pass from antimony to bismuth across the junction at J. The index I immediately moves towards the open end of the narrow tube, proving the air within the globe

FIG. 14.



to be expanded, or, in other words, heat to be developed by the passage of the current.

We now suddenly reverse the latter, causing it to flow from bismuth to antimony across the junction. The index I not only returns to its first position, but passes beyond it towards the angle formed by the tube. This retrocession of the index is due to the contraction of the air or, in other words, to the lowering of the temperature within the bulb. The experiment may be repeated any number

of times; always when the current passes from A to B we have expansion due to heat, and always when it passes from B to A we have contraction due to cold, of the air within the globe. The result enunciated by Peltier is thus made evident to you all.

Instead of a voltaic current, the thermo-electric current itself may be thus employed. Before me are two thermo-piles, so connected together that the current produced by warming the face of one of them can be sent through the other. We will allow the current to flow for a minute or so through the second pile. Breaking the connexion between the two piles, and connecting the second one with a delicate reflecting galvanometer, there is a prompt deflection of the luminous index from right to left. Breaking the connexion with the galvanometer, and restoring that between the piles, by crossing the wires we send a current through the second one in a reverse direction. Connecting this latter again with the galvanometer, a prompt deflection from left to right is the consequence. The changes of temperature at the two faces of the second pile, produced by the current sent through it from the first, are always in accordance with the law of Peltier.

We have thus far operated on the soldered bismuth and antimony by an extraneous current, and have now to converge our attention on that of the single thermo-pile with which our first experiments were made. When the face of the instrument is warmed, what, in relation to its two metals, is the direction of the current produced? It is invariably from bismuth to antimony across the junction; and the effect of this current, at the junction, is the same as that observed by Peltier. There is in both cases a consumption of heat.

There is, however, a production of heat at the opposite face of the pile, where the current passes from antimony to bismuth. But the heat there generated is less than

that consumed at the other junction. It is this excess of consumed heat that has been transmuted into the electric current that moved the needle, that tapped the mica, and that warmed the galvanometer coil. To use Mayer's simile, the whole process might be compared to one of distillation, where water after its conversion into steam passes from a boiler to a distant condenser, and reassumes there its primitive liquid form. The heat communicated to our pile may be figured as distilling into the subtler electric current, which latter is, as it were, recondensed to heat at a distance from its source. And, as in the case of real distillation, the liquid withdrawn as vapour from the boiler is found, as liquid, without loss in the condenser, so the heat consumed by our current at the warm face of the thermo-pile, is found undiminished, as heat, in the other portions of the circuit.[1]

[1] Peltier's result, which was reproduced in a striking manner by Lenz, is exhibited in a variety of forms in a brief paper in the 'Philosophical Magazine,' 1852, vol. iv. p. 419.

APPENDIX TO LECTURE I.

NOTE ON THE CONSTRUCTION OF THE GALVANOMETER.

The existence and direction of an electric current are shown by its action upon a freely suspended magnetic needle.

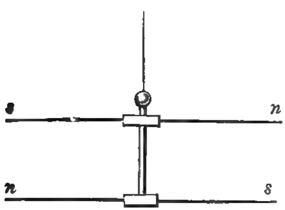
But such a needle is held in the magnetic meridian by the magnetic force of the earth. Hence, to move a single needle, the current must overcome the magnetic force of the earth.

Very feeble currents are incompetent to do this in a sufficiently sensible degree. The following two expedients are, therefore, combined to augment the action of such feeble currents:

The wire through which the current flows is coiled, so as to surround the needle several times; the needle must swing freely within the coil. The action of the single current is thus multiplied.

The second device is to neutralise the directive force of the earth, without prejudice to the magnetism of the needle. This is accomplished by using two needles instead of one, attaching them to a common vertical stem, and bringing their opposite poles over each other, the north end of the one needle and the south end of the other being thus turned in the same direction. The double needle is represented in fig. 15.

FIG. 15.



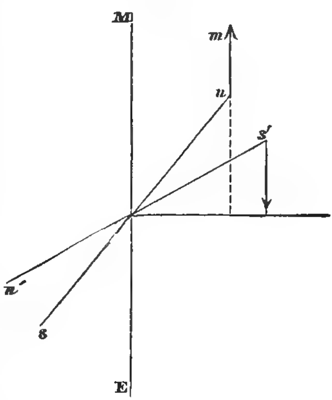
It must be so arranged that one of the needles shall be within the coil through which the current flows, while the other needle swings freely above the coil, the vertical connect-

ing piece passing through a slit in the coil. Were both the needles within the coil, the same current would urge them in opposite directions, and thus one needle would neutralise the other. But when one is within and the other without, the current urges both needles in the same direction.

The way to prepare such a pair of needles is this. Magnetise both of them to saturation: then suspend them in a vessel, or under a shade, to protect them from air-currents. The system will probably set in the magnetic meridian, one needle being in almost all cases stronger than the other; weaken the stronger needle carefully by the touch of a second smaller magnet. When the needles are precisely equal in strength, they will set at *right angles to the magnetic meridian.*

It might be supposed that when the needles are equal in strength, the directive force of the earth would be completely annulled, that the double needle would be perfectly *astatic,* and perfectly neutral as regards direction; obeying simply the torsion of its suspending fibre. This would be the case if the magnetic axes of both needles could be caused to lie with mathematical accuracy in the same vertical plane. In practice this is impossible; the axes always cross each other. Let $n\ s$, $n'\ s'$ (fig. 16) represent the axes of two needles thus crossing, the magnetic meridian being parallel to M E; let the pole n be drawn by the earth's attractive force in the direction $n\ m$; the pole s' being urged by the repulsion of the earth in a precisely opposite direction. When the poles n and s' are of exactly equal strength it is manifest that the force acting on the pole s', in the case here supposed, would have the advantage as regards leverage, and that it would therefore overcome the force acting on n. The crossed

FIG. 16.



needles would therefore turn away still farther from the magnetic meridian, and a little reflection will show that they cannot come to rest until the line which bisects the angle inclosed by the needles is at right angles to the magnetic meridian.

This is the test of perfect equality as regards the magnetism of the needles; but in bringing them to this state of perfection we have often to pass through various stages of obliquity to the magnetic meridian. In these cases the superior strength of one needle is compensated by an advantage, as regards leverage, possessed by the other. By a happy accident a single touch is sometimes sufficient to make the needles perfectly equal; but many hours are often expended in securing this result. It is only, of course, in very delicate experiments that this perfect equality is needed; but in such experiments it is essential.

Another grave difficulty has beset experimenters, even after the perfect magnetisation of their needles has been accomplished. Such needles are sensitive to the slightest magnetic action, and the covered copper wire, of which the galvanometer coils are formed, usually contains a trace of iron sufficient to deflect the prepared needle from its true position. I have had coils in which this deflection amounted to thirty degrees; and in the splendid instruments used by Professor Du Bois-Raymond, in his researches on animal electricity, the deflection by the coil is sometimes even greater than this. Melloni encountered this difficulty, and proposed that the wires should be drawn through agate holes, thus avoiding all contact with iron or steel. The disturbance has always been ascribed to a trace of iron contained in the copper wire. Pure silver has also been proposed instead of copper.

To pursue his beautiful thermo-electric researches in a satisfactory manner, Professor Magnus, of Berlin, obtained pure copper by a most laborious electrolytic process, and after the metal had been obtained it required to be melted eight times in succession before it could be drawn into wire. In fact, the impurity of the coil entirely vitiated the accuracy of the instruments, and almost any amount of labour would be well expended in removing this great defect.

My own experience of this subject is instructive. I had a beautiful instrument constructed for me a few years ago by Sauerwald of Berlin, the coil of which, when no current flowed through it, deflected my double needle fully thirty degrees from the zero line. It was impossible to attain quantitative accuracy with this instrument.

I had the wire removed by Mr. Becker, and English wire used in its stead; the deflection fell to three degrees.

This was a great improvement, but not sufficient for my purpose. I made various inquiries about the possibility of obtaining pure copper, but the result was very discouraging. When almost despairing, the following thought occurred to me: The action of the coil must be due to the admixture of iron with the copper, for pure copper is diamagnetic, and therefore feebly *repelled* by a strong magnet. The magnet therefore occurred to me as a means of instant analysis; I could tell by it, in a moment, whether my wire was free from magnetic metal or not.

The wire of M. Sauerwald's coil was strongly attracted by the magnet. The wire of Mr. Becker's coil was also attracted, though in a much feebler degree.

Both wires were covered with green silk: I removed this, but the Berlin wire was still attracted; the English wire, on the contrary, when presented *naked* to the magnet was feebly *repelled*; it was truly diamagnetic, and contained no sensible trace of iron. Thus the annoyance was fixed upon the green silk; some iron compound had been used in the dyeing of it, and to this the deviation of the needle was manifestly due.

I had the green coating removed and the wire overspun with white silk, clean hands being used in the process. A perfect galvanometer is the result; the needle, when released from the action of the current, returns accurately to zero, and is perfectly free from all magnetic action on the part of the coil. In fact, while we have been devising agate plates and other learned methods to get rid of the nuisance of a magnetic coil, the means of doing so are at hand. Let the copper wire be selected by the magnet, and no difficulty will be experienced in obtaining specimens magnetically pure.

LECTURE II.

NATURE OF HEAT—VIEWS OF BACON, DESCARTES, BOYLE, HOOKE, NEWTON, EULER, LOCKE, AND RUMFORD—RUMFORD'S EXPERIMENTS ON THE FRICTION OF IRON—DAVY'S EXPERIMENTS ON THE FRICTION OF ICE.

APPENDIX:—EXTRACTS FROM BACON.

THE development of heat by mechanical action was illustrated by suitable experiments when we last assembled here. But experimental facts alone cannot satisfy the mind: we desire to know the cause of the fact; we search after the principle by the operation of which the phenomena are produced. Why should heat be generated by mechanical action, and what is the real nature of the agent thus generated? History shows us two different philosophical schools trying to account for natural phenomena; the one resorting to speculation, the other to observation and experiment. The two schools, however, were not mutually exclusive, for the former required at least some of the data of observation to build upon, while the latter invoked the aid of speculation when the cement of principles was required to unite observed facts. In our day the school which emphasises experience has gained the upper hand. It is a common statement, indeed, that in investigating nature we cannot transcend experience, and properly qualified, the statement is true. Our conceptions of natural phenomena and their causes are founded on, but they are not bounded by, sensible experience. The eternally-falling atoms of Epi

curus and Lucretius, for example, were derived from the observation of small particles of matter; but in transforming such particles, by a mental act, into atoms, the ancient philosophers broke ground in an ideal region. The notion of *falling* indicates the manner in which the ancient mind was conditioned by experience; for in those days while the action of gravity was known, the action of molecular force, capable of attracting and arranging the atoms, was unknown. The case is representative, the visible world being converted by science into the symbol of an invisible one. We can have no explanation of the objects of experience, without invoking the aid and ministry of objects which lie beyond the pale of experience. We can only reach the roots of natural phenomena by laying down, intellectually, a subsensible soil out of which such phenomena spring.

HISTORIC NOTICES.

This tendency to explain the seen by reference to the unseen is continually manifested in the efforts of curious and penetrative minds to obtain a notion of the nature of Heat. They had constant recourse to the scientific imagination. Heat atoms and fire atoms were pictured as driving fiercely into the pores of bodies, loosening their molecules and shaking them asunder—thus reducing solids to liquids, and liquids to vapours. The notion that heat was a kind of motion was vaguely entertained by Plato, who makes Socrates say: 'For heat and fire which generate and sustain other things, are themselves begotten by impact and friction: but this is motion. Are not these the origin of fire?' The same thought was clearly formulated by Bacon, who defined heat to be 'a motion acting in its strife upon the smaller particles of bodies.' His illustrations of this motion were not, however, always

happy ones. Descartes, and others in his day, had a clear conception that the sensation of heat arose from a kind of motion communicated to the nerves; and some of these early writers were also clear as to the fact that the sensation was derived from the molecular motion of the warm body. They, however, for the most part assumed a special igneous matter, which produced the molecular motion. The illustrious Robert Boyle, for example, affirmed *heat* to be molecular motion, but to account for *fire* he assumed a special igneous matter. Euler and Newton curiously changed places with regard to their respective notions of light and heat. Euler was one of the most ardent defenders of the Undulatory Theory, which ascribes light to vibratory motion; but he regarded heat as a kind of matter. Newton supported the Emission Theory, which assumed light to be a kind of matter, while he considered heat to be vibratory motion. Hobbes, it may be added, was very distinct in his affirmation that heat is motion; and with regard to solar heat he avows his disbelief that anything material is emitted by the sun.

Robert Boyle appears to have seen as clearly as we do to-day, that when heat is generated by mechanical means *new heat* is called into existence.[1] In describing one of his experiments, he uses the following remarkable language: 'It will be convenient to begin with an instance or two of the production of heat, wherein there appears not to intervene anything in the part of the agent or patient but local motion, and the natural effects of it. When, for example, a smith does hastily hammer a nail or such like piece of iron, the hammered metal will grow exceedingly hot; and yet there appears not anything to make it so, save the forcible motion of the hammer, which impresses

[1] On this point Bacon also was perfectly clear. 'Heat,' he says, 'is produced by the motion of attrition, without any preceding heat.'

a vehement and variously determined agitation of the small parts of the iron, which, being a cold body before, by that superinduced commotion of its small parts, becomes in divers senses hot; first, in a more lax acceptation of the word in reference to some other bodies, in respect of whom it was cold before, and then sensibly hot; because this newly gained agitation surpasses that of the parts of our fingers. And in this instance, it is not to be overlooked, that oftentimes neither [both?] the hammer by which, nor [and?] the anvil on which a cold piece of iron is forged, continue cold after the operation is ended; which shows, that the heat acquired by the forged piece of iron was not communicated by the hammer or anvil as heat, but produced in it by motion, which was great enough to put so small a body, as the piece of iron, into a strong and confused motion of its parts, without being able to have the like operation upon so much greater masses of metal, as the hammer and the anvil. And now I am put in mind of an observation, that seems to contradict, but does indeed confirm our theory: namely, that if a somewhat large nail be driven by a hammer into a plank, or piece of wood, it will receive divers strokes on the head before it grows hot; but when it is driven to the head, so that it can go no farther, a few strokes will suffice to give it a considerable heat; for whilst at every blow of the hammer, the nail enters farther and farther into the wood, the motion that is produced is chiefly progressive, and is of the whole nail tending one way; whereas, when that motion is stopped, then the impulse given by the stroke, being unable either to drive the nail farther on, or destroy its entireness, must be spent in making a various, vehement and intestine commotion of the parts among themselves, and in such an one we formerly observed the nature of heat to consist.'

After 'the nimble hammering of iron by three lusty

men,' accustomed to the work, Boyle found the metal so hot that it could not be safely touched. To the wonder of the bystanders, it was able to ignite the sulphur of gunpowder and to cause it to burn with a blue flame. He also refers to the heat produced in cold iron, by a rough file causing an intestine commotion of its parts. Nothing can be clearer or more to the point than these utterances and illustrations.

Among the philosophers of the seventeenth century none, however, possessed a greater power of symbolising the phenomena of heat than Robert Hooke. His illustration of the manner in which fluidity is produced by the motion of heat is a fine example of his penetration. 'First,' he says, 'what is the cause of fluidness? This I conceive to be nothing else but a certain pulse or shake of heat; for heat being nothing else but a very brisk and vehement agitation of the parts of a body (as I have elsewhere made probable), the parts of a body are thereby made so loose from one another, that they easily move any way, and become fluid. That I may explain this a little by a gross similitude, let us suppose a dish of sand set upon some body that is very much agitated, and shaken with some quick and strong vibrating motion, as on a millstone turn'd round upon the under stone very violently whilst it is empty; or on a very stiff drum-head, which is vehemently or very nimbly beaten with the drumsticks. By this means the sand in the dish, which before lay like a dull and unactive body, becomes a perfect fluid; and ye can no sooner make a hole in it with your finger, but it is immediately filled up again, and the upper surface of it levell'd. Nor can you bury a light body, as a piece of cork under it, but it presently emerges or swims as 'twere on the top; nor can you lay a heavier on the top of it, as a piece of lead, but it is immediately buried in sand, and (as 'twere) sinks to the bottom. Nor can you make a hole in the side of

the dish, but the sand shall run out of it to a level. Not an obvious property of a fluid body, as such, but this does imitate; and all this meerly caused by the vehement agitation of the conteining vessel; for by this means, each sand becomes to have a vibrative or dancing motion, so as no other heavier body can rest on it, unless sustein'd by some other on either side: nor will it suffer any body to be beneath it, unless it be a heavier than itself.'

By this power of making the seen the symbol of the unseen, Hooke illuminated to a marvellous extent every subject touched by his genius. He refers to his own observations on the shining sparks of steel produced by a quick and violent motion, proving them to be perfectly similar to the sparks produced by heating the steel particles in a flame. Like Boyle, he also refers to the heating of iron by filing. He compares the vibrations of heat with sonorous vibrations, and adds the following remark: 'Now that the parts of all bodies though never so solid do yet vibrate, I think we need go no farther for proof than that all bodies have some degrees of heat in them, and that there has not yet been found anything perfectly cold.'

These notions regarding the nature of heat were generally prevalent among the scientific writers of the seventeenth century. They were also shared by philosophical writers. In his 'Essay on the Human Understanding,' Locke frequently refers to heat as being a kind of motion. But the very remarkable utterance which of late years has been most widely circulated is the following: 'Heat,' says Locke, 'is a very brisk agitation of the insensible parts of the object, which produces in us that sensation from whence we denominate the object hot; so what in our sensation is heat, in the object is nothing but motion. This appears by the way heat is produced; for we see that the rubbing of a brass nail upon a board

will make it very hot; and the axle-trees of carts and coaches are often hot, and sometimes to a degree, that it sets them on fire, by the rubbing of the naves of the wheels upon them. On the other side, the utmost degree of cold is the cessation of that motion of the insensible particles, which to our touch is heat.'[1]

Despite these utterances and arguments, so clear and, it might be added, so conclusive, in favour of the Mechanical or Dynamical Theory of Heat, its rival, the Material Theory, found a firm lodgment in many scientific minds. Within certain limits this theory involved conceptions of a very simple kind, and this simplicity secured for it a century ago universal acceptance. It was then assailed by Benjamin Thompson, better known as Count Rumford; but, despite his assault, it held its ground until quite recently among the chemists of our own day. The laborious Gmelin, for example, in his great 'Handbook of Chemistry,' defines heat to be that substance whose entrance into our bodies causes the sensation of warmth and its egress the sensation of cold. He also speaks of heat combining with bodies as one ponderable substance does with another, and I have frequently heard other eminent chemists treat the subject from the same point of view.

[1] In his excellent brochure entitled 'Rumford und die Mechanische Wärmetheorie,' Dr. Berthold remarks, with reference to a part of the foregoing quotation, that after having been first used by Dr. Joule in 1850, as a motto, and reproduced by myself, it has passed into all physical handbooks, without any writer giving himself the trouble to state the particular work of Locke in which it occurs. He finds that it was taken from an insignificant piece of writing, entitled the 'Elements of Natural Philosophy (London, 1722),' regarding which the publisher says, that Locke had dictated it for the use of a young gentleman in whose education he took great interest. I may add, that it is also to be found in vol. iv. p. 597 of Locke's works printed in 1768.

RUMFORD ON THE FRICTION OF IRON. DAVY ON THE FRICTION OF ICE.

With Rumford, however, a new and powerful factor appeared on the scene. He began by proving the hypothetical matter of heat to be imponderable, but the main drift of his experiments was to prove friction to be an inexhaustible source of heat, while the whole force of his logic went to show that an inexhaustible emission is irreconcilable with the notion that heat is a kind of matter. By those who held the material theory the matter of heat was supposed to hide itself in the interatomic spaces of bodies, out of which it could be squeezed by compression or percussion as water is squeezed from a sponge. They were acquainted with the fact (which shall be amply elucidated on a future occasion), that different bodies possess different powers of holding heat, if such a term may be employed. Take, for example, the two liquids, water and mercury, and warm a pound of each of them, say from fifty degrees to sixty. The absolute quantity of heat required by the water to raise its temperature ten degrees is fully thirty times the quantity required by the mercury. Technically speaking, water was said to have a greater *capacity* for heat than mercury, and this term 'capacity' suggests the views of those who invented it. Water was supposed to possess an enormous power of storing up caloric or the matter of heat;—of hiding heat, in fact, to such an extent that it required thirty measures of this caloric to produce the same sensible effect on it that one measure could produce upon the same weight of mercury.

All substances possess, in a greater or lesser degree, this apparent power of storing up heat. Lead, for example, possesses it; and our experiment with the lead bullet, in which heat was generated by compression, was explained by those who held the material theory in the following

way: The uncompressed lead, they said (without however proving what they said), has a higher capacity for heat than the compressed substance; the size of its atomic storehouse is diminished by compression, and hence, when the lead is squeezed, a portion of that heat which, previous to compression, was hidden, must make its appearance, for the compressed substance can no longer hold it all. In some similar way the experiments on friction and percussion were accounted for; the idea of calling *new heat* into existence being rejected by the believers in the material theory. According to their views, the quantity of heat in the universe is as constant as the quantity of ordinary matter, and the utmost we can do by mechanical and chemical means, is to store up this heat, or to drive it from its lurking places into the open day.

Such views were rudely shaken by the experiments and arguments of Rumford. Surprised by the degree of heat which a brass gun acquires in a short time on being bored, and the still more intense heat, 'much greater than boiling water, of the metallic chips separated from it by the borer;' he proposed to himself the following questions:

'Whence comes the heat actually produced in the mechanical operation above mentioned?

'Is it furnished by the metallic chips which are separated from the metal?'

If this were the case, tnen the capacity for heat of the parts of the metal so reduced to chips ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the heat produced. No such change, however, had taken place; for the chips were found to have the same capacity as slices of the same metal cut by a fine saw, where heating was avoided. Hence, it is evident, that the heat produced could not possibly have been furnished at the expense of the latent

heat of the metallic chips. Rumford describes these experiments at length, and they are conclusive.

He then designed a gun-metal cylinder for the express purpose of generating heat by friction. A blunt rectangular piece of hardened steel, called by Rumford a borer, was forced edgeways against the solid bottom of the cylinder, while the latter was turned round its axis by the force of horses. To measure the heat developed, a small round hole was bored in the cylinder, into which was introduced a small mercurial thermometer. The weight of the cylinder was 113·13 lbs. avoirdupois. The borer was 0·63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, 3½ inches. The area of the surface by which its end was in contact with the bottom of the bore was therefore nearly 2⅓ inches. At the beginning of the experiment the temperature of the air in the shade, and also that of the cylinder, was 60° F. At the end of 30 minutes, after the cylinder had made 960 revolutions round its axis, the temperature was found to be 130°.

Having taken away the borer, he now removed the metallic dust, or scaly matter, which had been detached from the bottom of the cylinder, and found its weight to be 837 grains troy. 'Is it possible,' he exclaims, 'that the very considerable quantity of heat produced in this experiment—a quantity which actually raised the temperature of above 113 pounds of gun-metal at least 70 degrees of Fahrenheit's thermometer—could have been furnished by so inconsiderable a quantity of metallic dust, and this merely in consequence of a change in its capacity for heat? But, without insisting on the improbability of this supposition, we have only to recollect that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat of the metal of which great guns are cast is *not sen-*

siblychanged by being reduced to the form of metallic chips, and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces by a borer which is less sharp.'

Rumford next surrounded his cylinder by an oblong deal box, so that the cylinder could turn water-tight in the centre of the box, while the borer was pressed against the bottom of the cylinder. The box was filled with water until the entire cylinder was covered, and then the apparatus was set in action. The temperature of the water on commencing was 60° Fahr.

'The result of this beautiful experiment,' writes Rumford, 'was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had in contriving and arranging the complicated machinery used in making it. The cylinder had been in motion but a short time, when I perceived, by putting my hand into the water, and touching the outside of the cylinder, that heat was generated

'At the end of one hour the fluid, which weighed 18·77 lbs., or 2½ gallons, had its temperature raised 47 degrees, being now 107 degrees.

'In thirty minutes more, or one hour and thirty minutes after the machinery had been set in motion, the heat of the water was 142 degrees.

'At the end of two hours from the beginning, the temperature was 178 degrees.

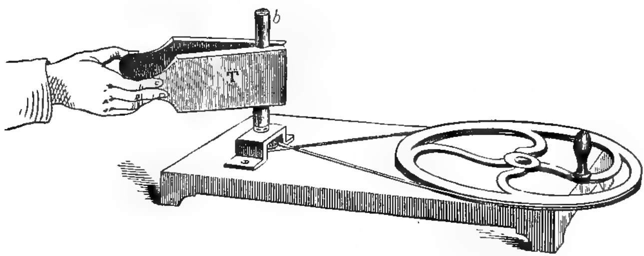
'At two hours and twenty minutes it was 200 degrees; and at two hours and thirty minutes it ACTUALLY BOILED!'

'It would be difficult,' says Rumford, 'to describe the surprise and astonishment expressed in the countenances of the bystanders on seeing so large a quantity of water heated and actually made to boil, without any fire. Though there was nothing that could be considered very surprising in this matter, yet I acknowledge fairly that it

afforded me a degree of childish pleasure which, were I ambitious of the reputation of a grave philosopher, I ought most certainly rather to hide than to discover.' [1] I am sure we can dispense with the application of any philosophy which would stifle such emotion as Rumford here avowed.

We cannot devote two hours and a half to a single experiment; but I hope to be able to show you substantially the same effect in two minutes and a half. This brass tube (*b*, fig. 17), four inches long, and three-quarters of an inch interior diameter, is stopped at the bottom, and screwed on to a whirling table, by

FIG. 17.



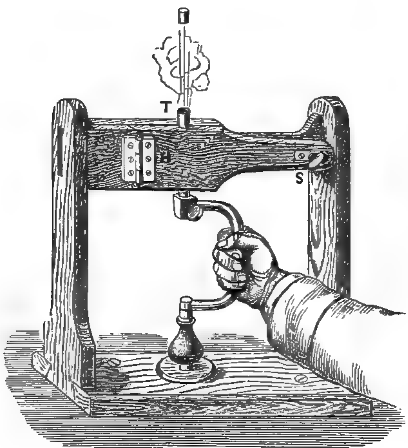
means of which it can be caused to rotate very rapidly. Two pieces of oak, with two semicircular grooves, are so united by a hinge as to form a kind of tongs, T, the gentle squeezing of which produces friction when the tube rotates. I partially fill the tube with cold water, stop it with a cork to prevent the splashing out of the liquid, and now put the machine in motion. The temperature of the water gradually rises, and the tube soon becomes too hot to be held in the fingers. Continuing the action a little longer, the cork is driven out with explosive violence, the steam which follows it producing by its precipitation a small cloud in the atmosphere.

[1] Rumford's Essays, vol. ii. p. 484.

In the absence of a whirling table, this experiment may be made with the bit-and-brace arrangement shown in fig. 18. The tube T, containing the water to be boiled, is clasped by the wooden tongs H, which can be tightened to any required extent by the screw S. After a brief period of rotation, steam is generated and the cork is ejected. Though less simple, the whirling table is, however, more effectual.

Using a more volatile liquid than water, we may dispense with the wooden tongs. Filling our tube partially

FIG. 18.



with sulphuric ether, and clasping it with the hand protected by a woollen or buckskin glove, a very brief rotation suffices to raise the vapour of the ether to a tension sufficient to propel the cork against the ceiling of this room.

Rumford carefully estimated the quantity of heat possessed by each portion of his apparatus at the conclusion of his experiment, and adding all together, found a total sufficient to raise 26·58 lbs. of ice-cold water to its

boiling-point, or through 180 degrees Fahrenheit. By careful calculation, he found this heat equal to that given out by the combustion of 2,303·8 grains (= $4\frac{8}{10}$ oz. troy) of wax. He then determined the '*celerity*' with which the heat was generated, summing up thus: 'From the results of these computations, it appears that the quantity of heat produced equably, or in a continuous stream, if I may use the expression, by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, was *greater* than that produced in the combustion of nine *wax candles*, each three-quarters of an inch in diameter, all burning together with clear bright flames.'

'One horse,' he continues, 'would have been equal to the work performed, though two were actually employed. Heat may thus be produced merely by the strength of a horse, and, in a case of necessity, this heat might be used in cooking victuals. But no circumstances could be imagined in which this method of procuring heat would be advantageous; for more heat might be obtained by using the fodder necessary for the support of a horse as fuel.'

This is an extremely significant passage, intimating, as it does, that Rumford saw clearly that the force of animals was derived from the food: *no creation of force* taking place in the animal body.

'By meditating on the results of all these experiments, we are naturally,' he says, 'brought to the great question which has so often been the subject of speculation among philosophers, namely, What is heat—is there any such thing as an *igneous fluid*? Is there anything that, with propriety, can be called caloric?'

'We have seen that a very considerable quantity of heat may be excited by the friction of two metallic surfaces, and given off in a constant stream or flux *in all directions*, without interruption or intermission, and without any signs of *diminution* or *exhaustion*. In reasoning

on this subject, we must not forget *that most remarkable circumstance*, that the source of the heat generated by friction in these experiments appeared evidently to be *inexhaustible*. [The italics are Rumford's.] It is hardly necessary to add, that anything which any *insulated* body or system of bodies can continue to furnish *without limitation* cannot possibly be a *material substance*; and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated in those experiments, except it be MOTION.'

With regard to the illustration which compared heat to water contained in a sponge, Rumford replied thus: 'A sponge filled with water and hung by a thread in the middle of a room filled with dry air communicates its moisture to the air it is true, but soon the water evaporates and the sponge can no longer give out moisture.' The case, he contended, is not at all similar to heat; for here, by renewed mechanical action, we can cause the heat to flow out at will. 'A bell,' he says, 'sounds without intermission when it is struck, and gives out its sound as often as we please, without any perceptible loss. Moisture is a substance, sound is not.' Heat, he contended, was typified by the vibrating bell and not by the evaporating sponge.

The conclusion drawn from these experiments by Rumford was contested by Bertholet, who stood forth as the champion 'of the received theory of caloric.' His arguments were fully set forth by Rumford, and totally overthrown. When the history of the dynamical theory of heat is completely written, the man who, in opposition to the scientific belief of his time, could experiment, and reason upon experiment, as Rumford did in the investigation here referred to, may count upon a foremost place. Hardly anything more powerful against the materiality of heat

has been since adduced, hardly anything more conclusive in the way of establishing that heat is, what Boyle, Hooke, and Locke considered it to be, *Motion*.

And here we may refer to an observation of Rumford's, which indicates at once his penetration and the limit of his knowledge. In 1778 he was engaged in experiments on the force of gunpowder, employing a musket barrel which he sometimes fired without any bullet, and sometimes with one, two, three, or even four bullets. Immediately after each discharge it was his practice to seize the barrel in his hand, while it was wiped out, and he was astonished to notice that the barrel was always hotter when the charge consisted of powder alone, than when loaded with one or more bullets. Rumford rejected the notion that the gun was heated by the flame of the gunpowder, which he considered far too transitory to produce the heating effect observed. He referred that effect to mechanical concussion. Assuming heat to be 'a more or less rapid vibratory motion among the particles of solid bodies,' he concluded that when the powder alone was fired, the shock was 'more vibrating or heavier' than when the combustion was obliged 'to push slowly before it one or two balls which were anything but light.' Had Rumford been aware of the entire bearing of the mechanical theory of heat, he would not, I think, have omitted to mention, in connexion with this experiment, that the gunpowder urging the ball could not possibly generate the same amount of heat as when urging no ball. Rumford omitted all allusion to this; and Mayer was the first to discern the meaning of his observation.

Stimulated probably by Rumford, with whom he was personally connected at the Royal Institution, Davy took up this subject, and enriched it by a beautiful and conclusive experiment.[1] Ice is solid water, and the solid has

[1] Works of Sir H. Davy, vol. ii. p. 11.

only one-half the capacity for heat that liquid water possesses. A quantity of heat which would raise a pound of ice ten degrees in temperature would raise a pound of water only five degrees. Further, simply to liquefy a mass of ice, an enormous amount of heat is necessary, this heat being so utterly absorbed or rendered 'latent' as to make no impression upon the thermometer. The question of 'latent heat' shall be fully discussed in its proper place: what I am desirous of impressing on you at present is, that, taking the materialists on their own ground, *liquid water*, at its freezing temperature, possesses a vastly greater amount of heat than *ice* at the same temperature.

Davy reasoned thus: 'If I, by friction, liquefy ice, a substance will be produced which, according to the material theory, contains a far greater absolute amount of heat than the ice. In this case, it cannot with any show of reason be affirmed that I merely render sensible heat which had been previously insensible in the frozen mass. Liquefaction will conclusively demonstrate a generation of new heat.' He made the experiment, and liquefied the ice by pure friction. The experiment has been justly regarded as fatal to the material theory.[1]

[1] Davy's experiment was in some measure anticipated by Sir Thomas Brown, 'Pseudodoxia Epidemica,' Book II. chap. i. 'Again the concretion of ice will not endure a dry attrition without liquation, for if it be rubbed long with a cloth it melteth.'

Thomas Young saw clearly the significance of the foregoing results and concluded that the experiments of Rumford and Davy afforded 'an unanswerable confutation' of the material theory of heat. 'There is no alternative,' he contends, 'but to allow that heat must be actually generated by friction. . . . If heat,' he adds, 'be not a substance, it must be a quality; and this quality can only be a motion.' Cavendish took the same view. Maxwell, 'Theory of Heat' page 72.

APPENDIX TO LECTURE II.

EXTRACTS FROM THE TWENTIETH APHORISM OF THE SECOND BOOK OF THE 'NOVUM ORGANUM.'

When I say of motion that it is the genus of which heat is a species, I would be understood to mean, not that heat generates motion, or that motion generates heat (though both are true in certain cases), but that heat itself, its essence and quiddity, is motion and nothing else; limited, however, by the specific differences which I will presently subjoin, as soon as I have added a few cautions for the sake of avoiding ambiguity. . . . .

Nor, again, must the communication of heat, or its transitive nature, by means of which a body becomes hot when a hot body is applied to it, be confounded with the form of heat. For heat is one thing, and heating is another. Heat is produced by the motion of attrition without any preceding heat. . . . .

Heat is an expansive motion, whereby a body strives to dilate and stretch itself to a larger sphere or dimension than it had previously occupied. This difference is most observable in flame, where the smoke or thick vapour manifestly dilates and expands into flame.

It is shown also in all boiling liquid, which manifestly swells, rises and bubbles, and carries on the process of self-expansion, till it turns into a body far more extended and dilated than the liquid itself, namely, into vapour, smoke, or air.

. . . .

The third specific difference is this, that heat is a motion of expansion, not uniformly of the whole body together, but

in the smaller parts of it; and at the same time checked, repelled and beaten back, so that the body acquires a motion alternative, perpetually quivering, striving and struggling, and irritated by repercussion, whence springs the fury of fire and heat.

Again, it is shown in this, that when the air is expanded in a calender glass, without impediment or repulsion, that is to say uniformly and equably, there is no perceptible heat. Also when wind escapes from confinement, although it burst forth with the greatest violence, there is no very great heat perceptible; because the motion is of the whole, without a motion alternating in the particles.

And this specific difference is common also to the nature of cold; for in cold contractive motion is checked by a resisting tendency to expand, just as in heat the expansive action is checked by a resisting tendency to contract. Thus, whether the particles of a body work inward or outward, the mode of action is the same.

. . . .

Now from this our first vintage it follows, that the form or true definition of heat (heat that is, in relation to the universe, not simply in relation to man) is, in a few words, as follows: *Heat is a motion, expansive, restrained, and acting in its strife upon the smaller particles of bodies.* But the expansion is thus modified: *while it expands all ways, it has at the same time an inclination upwards.* And the struggle in the particles is modified also; *it is not sluggish, but hurried and with violence.*[1]

[1] Bacon's Works, vol. iv.: Spedding's Translation.

LECTURE III.

CHEMICAL HEAT, COMBUSTION AND FERMENTATION—PHYSIOLOGICAL HEAT—DISAPPEARANCE OF HEAT IN CHEMICAL PROCESSES—FLAME—COMBUSTION ON MONT BLANC—COMBUSTION OF AIR IN COAL-GAS—ELECTRICAL HEAT—SPARK OF ELECTROPHORUS AND ELECTRIC MACHINE—HEAT OF MAGNETO-ELECTRIC INDUCTION—FUSION OF METAL IN MAGNETIC FIELD—HEAT OF THE VOLTAIC BATTERY—MUSCULAR HEAT IN RELATION TO MUSCULAR WORK.

CHEMICAL HEAT.

WE have amply illustrated the production of heat by mechanical means. But heat is most usually produced by chemical means; and we must learn very rapidly, but very thoroughly, to-day, what this signifies when viewed from the standpoint of the dynamical theory.

We will begin at the very basis of the phenomena of combustion. On the plate of an air-pump I place a bit of burning candle, and over the candle a large glass receiver. Exhausting the receiver, the flame gradually becomes dim. It swells in size, and at last hardly yields any light. Permitting air to enter the receiver, it at once revives. Again exhausting, I allow the flame to go entirely out; leaving, however, a glowing ember at the tip of the wick. Rapidly permitting a little oxygen to enter the receiver, the candle is reignited and burns with vivid brilliancy. The candle cannot burn without air, and when fed by oxygen instead of air its combustion is vastly intensified.

One-fifth part by volume of our atmosphere is composed cf this vivifying gas, the other four-fifths being nitrogen,

which does not minister to the purposes of combustion. The energy of combustion is diluted by a substance which takes its heat, and yields no return. I take from the fire a piece of half-burnt coal, and urge against it from this iron bottle, in which the gas is compressed, a stream of pure oxygen. It burns with dazzling splendour. In a suitable flame I heat a diamond to vivid redness, and plunge it rapidly into a jar of oxygen. It glows there like a pure white star. A properly prepared fragment of coke is not to be distinguished in this respect from the diamond; both of them are carbon, and in oxygen they behave alike.

It is not, however, necessary that the oxygen should be presented to the carbon in the form of a gas: combustion also takes place when the oxygen is so locked up by other bodies as to form a solid. Gunpowder, for example, carries its own store of solid oxygen. In saltpetre we have a large quantity of the substance combined with nitrogen and potassium. Behind me, over a gas-flame, is a basin containing fused saltpetre. Throwing into the dish a fragment of the ignited bark charcoal, with which Faraday loved to operate on account of the beauty of its scintillations, a miniature volcano of sparks is the consequence.

How are we to present to our minds the burning of a diamond in oxygen? I have already briefly alluded to the theory which pictures meteoric masses raining down upon the sun, and generating by their collisions solar light and heat. Reduce the sun, in idea, to the magnitude of our diamond, and the meteoric masses to the magnitude of our atoms of oxygen, and you have an image of what occurred a moment ago in this jar. The diamond is a closely packed assemblage of carbon atoms, between which, when heated, and the surrounding oxygen there is exerted an enormous attractive force. Urged by this force, upon every carbon atom the oxygen atoms precipitate

themselves in pairs, two atoms of oxygen conspiring as it were to attack each single atom of carbon. The heat of this atomic collision is so intense that the carbon, in union with the oxygen, is jerked away from the solid nucleus, yielding a substance which is neither carbon nor oxygen, but a gaseous compound of both, called carbonic acid.

When atoms thus combine chemically in groups, the groups are called 'molecules.' Every molecule of carbonic acid consists then of a group of three atoms, of which two are oxygen and one is carbon. This substance is locked up in great quantities in chalk and marble, both of which are compounds of carbonic acid and lime. Now in these chemical affairs, as in others, the strong dislodges the weak and takes its place, so that if we pour upon chalk, or marble, an acid stronger than carbonic acid, it will displace the latter and set it free as a gas.

In carbonic acid the mutual attraction of oxygen and carbon being satisfied, the heat-producing power is used up, and combustion can no longer be sustained. The gas is heavier than air, and it may therefore be collected in an open jug. Plunging a lighted taper into a jug thus filled, it is immediately extinguished. Pouring the gas like water from the jug upon a candle-flame, you see nothing descending, but the extinction of the flame proves that something inimical to its existence has fallen upon it. Blowing a soap-bubble and tossing it upon the heavy gas contained in a suitable vessel, the bubble is arrested, and, after a few pendulous up-and-down movements, it floats at rest upon an invisible fluid. Permitting the light from our electric lamp to fall upon a large white screen, and pouring out the gas in front of the lamp, it is seen, falling as a cascade down the screen. In the Grotto del Cane, near Naples, this gas, produced naturally, forms an undercurrent, or pool, about a foot in depth, and the brutal ex-

periment is constantly made of pressing the head of a little dog underneath the gas until the creature becomes insensible. Neither light nor animal life can be supported by this substance.

The fermentation in a brewery is caused by the growth of a little fungus, which consumes oxygen and produces carbonic acid. The putrefaction of meat or soup is wholly due to the multiplication of extremely minute organisms, which generate among other products carbonic acid. This flask, for example, contained a few days ago a perfectly transparent infusion of beef. It was corked up, and in two or three days it became muddy as you now see it, the muddiness being due to innumerable swarms of microscopic organisms, which are now darting to and fro through the liquid. They first consumed the oxygen dissolved in the liquid, the place of which was taken by the oxygen in the air above the liquid. This also is now consumed, its place being taken by carbonic acid. Removing the cork from the flask, I plunge into it a lighted taper—the flame is immediately extinguished.

PHYSIOLOGICAL HEAT.

This leads me to say a word or two regarding our own warmth. During the whole course of our lives we are continually inhaling and exhaling atmospheric air. Now the nitrogen, which, as we have already learnt, constitutes four-fifths of the bulk of our atmosphere, does nothing towards the support of life. It is solely its companion element that sustains us. When we inhale, the oxygen passes across the cell-walls of the lungs and mixes with the blood, by which it is carried through the body. When we exhale, we pour out from the lungs the carbonic acid produced by the slow combustion of our bodies. To this

slow combustion we owe our animal heat. Carbonic acid may be regarded as the *rust* of the body, which is continually cleared away by the lungs.

In every part of the body this combustion is going on. The blood is forced by the heart through the arteries to all parts of the system, and, after passing through the capillaries, it returns to the heart through the veins. The venous blood is much darker than the arterial blood, an effect due to the de-oxidation of the blood. To make room for fresh oxygen, the black venous blood yields up in the lungs the carbonic acid with which, through the combustion of the body, it was previously charged, the red colour being thus restored.

Consider, then, all the fires in the world and all the animals in the world continually pouring their carbonic acid into the atmosphere. Would it not be fair to conclude that our air must become more and more contaminated, and unfit to support either combustion or life? This seems inevitable, but it would be a conclusion founded upon half knowledge, and therefore wrong. A provision exists for continually purifying the atmosphere of its excess of carbonic acid. By the leaves of plants this gas is absorbed, and within the leaves it is decomposed by the solar rays. The carbon is stored up in the tree, while the pure oxygen is restored to the atmosphere. Carbonic acid, in fact, is to a great extent the nutriment of plants; and inasmuch as animals, in the long run, derive their food from the vegetable world, this very gas, which at first sight might be regarded as a deadly constituent of the atmosphere, is the main sustainer both of vegetable and animal life.

That the air which comes from the lungs is different in quality from that which goes into them may be shown by a simple experiment. Carbonic acid is warm, and therefore light, when freshly exhaled. It does not readily fall

to the bottom of a vessel into which we breathe. But if the breath be chilled by sending it through a metal tube which passes through cold water, the carbonic acid may be collected in an open jar. A single expiration from the lungs suffices to fill a good-sized jar with the gas, which immediately quenches a lighted taper.

I have named the carbonic acid of our bodies 'rust,' and the reason I have done so is, that it is produced by the oxidation of carbon, as iron rust is produced by the oxidation of iron. This latter process is exactly analogous to the slow combustion within the animal frame; and when the heat thus produced is prevented from wasting itself, it may rise to destructive intensity. By such heat, in all probability, the first Atlantic cable was rendered useless. In 1861, the Messrs. Siemens had charge of the Rangoon and Singapore telegraph cable. Suspecting the injury that might accrue from heat, they had placed in the heart of each cable-coil an instrument capable of indicating any exaltation of temperature. The surmised increase occurred, the temperature augmenting daily by about 3° Fahr. A temperature of 86° was at length shown within the coil, when the outside temperature was only 60°. The cable would have been inevitably destroyed in the course of a few days, if the generation of heat had been allowed to continue unchecked. The cable was cooled by pouring water at a temperature of 42° Fahrenheit upon the top of the coil. It issued raised to 72° t the bottom.

Casting a backward glance over the series of actions here illustrated, we first figure the mutually attracting atoms apart, then rushing together, and acquiring while crossing the insensible interval which separates them the velocity with which they strike each other. That this velocity is enormous is proved by the amount of heat which it generates. This shall be made clearer by-and-by

When the atoms clash they recoil, and the consequent tremulous motion is one form of heat. Thus every molecule is animated by a vibratory motion of its constituent parts. It is a musical instrument complete in itself, the tremors of which, when they impinge on the nerves, produce the *sensation* of heat.

DISAPPEARANCE OF HEAT IN CHEMICAL PROCESSES.

Let us now fix our attention on the experiments with solid carbon, and oxygen in the state of gas—say the combustion of the diamond in oxygen. The amount of heat here generated is accurately known. Communicated to water, it would raise a weight of that liquid 7770 times the weight of the diamond 1° Centigrade in temperature. The heat generated by the other forms of carbon do not differ widely from this. The heat, for example, derived from the combustion of a pound of wood-charcoal would, according to Lavoisier, raise the temperature of 7237 pounds of water 1° Centigrade; while, according to Favre and Silbermann, it would produce this increase of temperature in 8080 pounds of water. Turn your thoughts now to the experiment with charcoal and saltpetre, where the violence of the combustion proved the intensity of the heat. Would a pound of charcoal thus burnt in saltpetre produce the same amount of heat as when burnt in free oxygen? No. Saltpetre, or nitrate of potash, is formed by the combination of nitrogen, potassium, and oxygen; one consequence of that combination being the generation of heat. To unlock the atomic embrace of the nitrogen, potassium, and oxygen, an amount of heat must be expended equal to that generated by their union; and by this exact amount the heat produced by combustion in saltpetre would fall short of that produced by combustion in free oxygen. In

the one case, we have the total heat produced by the union of carbon and oxygen; in the other case, we have that heat made less by the quantity necessary to effect the decomposition of the saltpetre.

Again, as regards our chalk and marble, their formation implies the generation and waste of enormous stores of heat. First, carbon and oxygen combined to form carbonic acid; secondly, calcium and oxygen combined to form lime; and, thirdly, carbonic acid combined with lime to form carbonate of lime. At each step of the process heat was generated. It was dispersed ages ago in space. In our experiment made a moment ago, we dislodged the carbonic acid from the marble. Now the stronger acid which effected this decomposition could unite with lime directly and generate heat. Is the heat observed in our experiment with the marble equal to that produced by the direct union of the stronger acid and the lime? No. It falls short of that heat by the quantity necessary to tear away the carbonic acid, which quantity is exactly that generated when the carbonic acid and the lime first rushed into union.

We now pass from the combustion of carbon in oxygen to the combustion of hydrogen in the same gas. Hydrogen is conveniently stored in strong iron bottles, and the one now before you contains at least thirty atmospheres of the compressed gas. Hydrogen is produced, as you know, by the action of zinc upon acidulated water. The oxygen of the water is attracted by the zinc, which becomes oxidized, the oxide being dissolved by the acid and fresh metallic surfaces continually exposed. The liberated hydrogen escapes as a gas. From our iron bottle I allow a jet of the gas to issue. On ignition it burns with a feebly luminous flame. Were the heat of a flame expressed by its light this would be a cool one. The heat, however, is not expressed by the light, and this

pale flame is capable of raising a refractory solid body to an intense white heat. A platinum wire, for example, placed in the flame is raised to vivid incandescence. But the flame itself must be hotter than the wire which it causes to glow. We have here, however, by no means the full intensity of the heat due to the combustion of oxygen and hydrogen. The atmospheric oxygen is diluted by four times its bulk of nitrogen, which weakens the combustion and robs the flame of heat. On substituting pure oxygen for atmospheric air, we obtain a vastly intensified action. By a proper apparatus we mix the two gases and ignite the mixture as it issues from a small nozzle. When this lightless flame impinges on a thin plate of platinum, which is one of the most refractory of metals, the flame first raises the platinum to a dazzling white heat, and then pierces it like a spear. Causing the flame to impinge upon a cylinder of lime, we have the brilliant Drummond light. It will afterwards be explained how a flame which itself is scarcely visible, and which, when examined by a prism, yields a barely perceptible trace of a spectrum, can, when it impinges upon a solid, produce a light so effulgent that its spectrum compares in richness with that of the sun.

When oxygen and hydrogen are caused to combine, the product of their union is pure aqueous vapour. Holding a cold glass surface above a hydrogen flame the glass is instantly dimmed by the condensation of the vapour. If the flame be caused to impinge upon a surface kept permanently cold, the condensed vapour may be collected as water. The bottle of water now held in my hand has been produced by a hydrogen flame which was caused to impinge upon the bottom of a silver basin filled with ice. When I first determined the boiling temperature upon the summit of Mont Blanc, my spirit-lamp was put out three or four times by the water of condensation. The boiler of

my apparatus was filled with snow, during the melting of which the condensation of the vapour of the lamp was incessant, and from time to time a heavy drop falling from the bottom of the boiler upon the wick of the lamp completely extinguished it.

Our illustrations of combustion might be extended indefinitely. Besides the intense incandescence of charcoal and the white glow of the diamond, I might show you the dazzling radiation of phosphorus, the scintillations of steel, or the purple and blue flames of zinc and sulphur when burnt in oxygen. With a clear image of the clashing of the atoms in a single case, you have, however, an image of what occurs in all. When the affinity is strong, and the store of oxygen sufficient, bodies burn in liquids as well as in gases. You have, indeed, seen that carbon not only burns in oxygen, but in fused saltpetre. From white sticks of potash, like those in this bottle, Davy extracted the metal potassium. I throw a pellet of the metal on water; it moves to and fro over the surface —for it is lighter than water—accompanied by a purple flame. It has combined with the oxygen of the water, dislodged the hydrogen, and produced heat intense enough to ignite the liberated gas. I place the metal in a little wire cage, and plunge it suddenly under water. There is now no flame, but copious bubbles of hydrogen rise from the cage. The hydrogen is here liberated where there is no free oxygen to burn it, and therefore we have no flame. The metal sodium, extracted by Davy from soda, has a less powerful affinity for oxygen than potassium has. It decomposes water—that is to say, it burns therein, liberates the hydrogen, but does not produce flame. Zinc acts similarly; it also burns in water; and, in connexion with the Voltaic battery, this form of combustion is of special importance. Placed in pure water, zinc immediately ceases

to decompose the liquid. It covers itself with a coating of oxide of zinc, which interrupts the contact between it and the adjacent oxygen. By adding a little sulphuric acid to the water, the oxide is dissolved, fresh metallic surfaces are continually exposed, and the decomposition then goes on without interruption.

In all these cases, however, the principle of equivalence is rigidly enforced. Davy could not have severed potassium from oxygen without the sacrifice of an equivalent amount of heat. The quantity of heat sacrificed is, moreover, exactly that generated by the re-formation of potash, through the combustion of the potassium. Again, the heat of our potassium pellet, when burnt on water, is not that which would be produced by the combustion of potassium in free oxygen gas. It falls short of this heat by the quantity necessary to sever the hydrogen of the water from the oxygen. This quantity of heat is exactly restored when the hydrogen takes fire, and re-burns to water. So also as regards zinc. In pure oxygen every pound of this metal generates by its combustion say n units of heat; that is to say, it would raise n pounds of water 1° in temperature. Burnt in dilute sulphuric acid, the same zinc yields only $n-y$ units of heat; the quantity y being sacrificed to effect the decomposition of the water.

FLAME.

We will now devote a few minutes to the examination of the flames which we employ for illumination, taking as our first illustration an ignited jet of gas. What is the constitution of that jet? Within the flame we have a core of gas as yet unburnt, and outside the flame we have the oxygen of the air. Between both is a space

in which the atoms clash together and produce light and heat by their collision. But the exact constitution of the flame is worthy of our special attention, and for our knowledge of this we are indebted to one of Davy's most beautiful investigations Coal-gas is what we call a hydro-carbon; it consists of carbon and hydrogen in a state of chemical union. From this transparent gas escape soot and lampblack when the combustion is incomplete. Soot and lampblack are there now, but they are compounded with other substances to a transparent form. This compound gas is now in contact with the oxygen of our air: we apply heat, and the attractions are instantly so intensified that the gas bursts into flame. The oxygen has a choice of two partners, and it first closes with that for which it has the strongest attraction. It unites with the hydrogen, and sets the carbon free. Innumerable solid particles of carbon thus scattered in the midst of the burning hydrogen are raised to a state of intense incandescence; they become white-hot, and mainly to them, according to Davy, the *light* of our lamps is due. The carbon however, in due time, closes with the oxygen, and becomes, or ought to become, carbonic acid; but in passing from the hydrogen with which it was first combined, to the oxygen with which it enters into final union, it exists for a moment in the solid state. Every solid particle rises, I may add, with immense rapidity through the flame, describing a white-hot *line*, and the sum of these lines gives us the light of the flame.

Fig. 19.

The combustion of a candle is in principle the same as that of a jet of gas. Here we have a rod of wax or tallow (fig. 19), through which passes a cotton wick. We ignite the wick; it burns, melts the tallow at its base, and the liquid ascends through the wick by capillary attraction. It is converted by the heat into vapour which burns exactly like coal-gas. In this case also we have unburnt vapour within, common air without, while between both is a shell, which forms the battle-ground of the clashing atoms, where they develop their light and heat.

In the case of a candle, as in that of gas, we have a hollow cone of burning matter. Imagine this cone cut across horizontally and looked down upon; a burning ring would be exposed. We can practically cut the flame of a candle thus across. I bring a piece of white paper down upon the candle, until it almost touches the wick. The upper surface of the paper becomes charred—how? Corresponding to the burning ring of the candle, we have a charred ring upon the paper (fig. 20). Operating in the

FIG. 20.

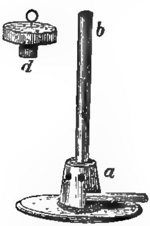
same manner with a jet of gas, we obtain the ring which it produces. Within the ring the paper is intact, for at this place the unburnt vapour of the candle, or the unburnt gas of the jet, impinges against the surface, and no charring can occur.

The existence of carbon particles, in a flame, implies the absence of oxygen to seize hold of them. If, at the moment of their liberation from the hydrogen, oxygen were present to seize upon them, their state of singleness would be abolished, and we should no longer have their

light. For this reason, when we mix a sufficient quantity of air with the gas issuing from a jet, and thus cause the oxygen to penetrate to the very heart of the jet, the light practically disappears.

Professor Bunsen has invented a burner for the express purpose of destroying, by quick combustion, the solid carbon particles. The burner from which the gas escapes is introduced into a tube or chamber, which is perforated nearly on a level with the orifice of the burner. Through the holes the air enters, mingles with the gas, and the mixture issues from the top of the tube. Fig. 21 represents a form of this burner. The gas is discharged into the perforated chamber *a*, where air mingles with it, and both ascend the tube *a b* together; *d* is a rose-burner, which may be used to vary the shape of the flame. I ignite the mixture, but the flame produces hardly any light. Heat is the thing here aimed at; and this lightless flame is much hotter than an ordinary flame, because the combustion is much quicker, and therefore more intense.[1] When the orifices in *a* are stopped, the supply of air is cut off, and the flame at once becomes luminous: we have now the ordinary case of a core of unburnt gas surrounded by a burning shell. The illuminating power of a gas may, in fact, be estimated by the quantity of air necessary to prevent the precipitation of the solid carbon particles; the richer the gas, the more air will be required to produce this effect.

FIG. 21.



To determine the influence of height upon the rate of combustion, was one of the problems which I set before myself in my journey to the Alps in 1859. On that occasion I invited Dr. Frankland to accompany me, and to undertake

[1] Not hotter, nor nearly so hot, to a body exposed to its radiation; but very much hotter to a body *plunged in the flame.*

the experiments on Combustion, while I devoted myself to observations on Solar Radiation. The plan pursued was this: six candles were purchased at Chamouni and carefully weighed; they were then allowed to burn for an hour in the Hôtel de l'Union, and the loss of weight was determined. The same candles were taken to the summit of Mont Blanc, and, on the morning of August 21, 1859, were allowed to burn for an hour in a tent, which shaded them from the sun and sheltered them from the wind. The aspect of the six flames at the summit surprised us both. They seemed the mere ghosts of the flames produced at Chamouni—enlarged, pale, feeble, and suggesting a greatly diminished energy of combustion. The candles being carefully weighed on our return, the unexpected fact was revealed, that the quantity of stearine consumed above was almost precisely the same as that consumed below. Thus, though the light-giving power of the flame was diminished in an extraordinary degree, the rapidity of the combustion was unchanged. This curious result is to be ascribed mainly to the mobility of the air at this great height. The particles of oxygen could penetrate the flame with comparative freedom, thus destroying its light, and making atonement for the smallness of their number by the rapidity of their action. I find, indeed, that by reducing the density of ordinary atmospheric air to one-half, we nearly double the mobility of its atoms.

Dr. Frankland has made these experiments the basis of a very interesting memoir.[1] He shows that the quantity of a candle consumed in a given time is, within wide limits, independent of the density of the air; and the reason is, that although by compressing the air we augment the number of active particles in contact with the flame, we, almost in the same degree, diminish their mobility and retard the combustion. One of the most interesting facts

[1] Philosophical Transactions for 1861.

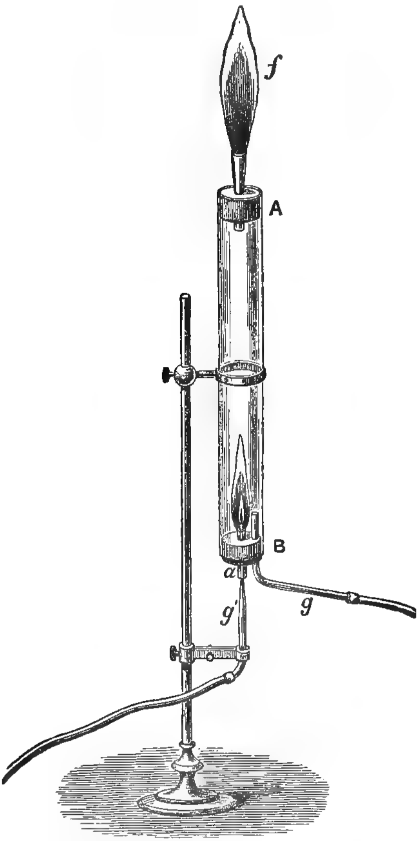
established by Dr. Frankland is, that by condensing the air around it, the pale and smokeless flame of a spirit-lamp may be rendered as bright as that of coal-gas, and, by pushing the condensation sufficiently far, the flame may actually be rendered smoky, the oxygen present being too sluggish to effect the complete combustion of the carbon.

In all these cases the heat and light produced are due to the union of two different substances, one being just as necessary as the other to the production of the result. We sometimes hear coal-gas spoken of as a combustible substance, and oxygen as a supporter of combustion; but if our atmosphere were composed of coal-gas, and if the great gas-holders at our gas-works were filled with oxygen, we should be able to burn that oxygen in an atmosphere of coal-gas. Or if, instead of being filled with oxygen, the gas-holders were filled with common air, we should be able to burn the air. The late Mr. Wills devised an arrangement by which this is readily accomplished. It is shown in fig. 22, where A B is a wide glass tube filled with coal-gas through the tube *g*. We make sure that it is full by igniting the gas at *f*. The air enters through the short tube *a*, which is open at the bottom. It is ignited by a small gas-flame issuing from the burner *g*′, which is inserted into *a*. The small gas-flame is shown in the figure within the larger air-flame. It may be withdrawn when the air is ignited.

To account for the propagation of fire was one of the difficulties of the last century. A spark was found sufficient to initiate a conflagration. The effect here seemed beyond all proportion greater than the cause, and herein lay the philosophical difficulty. By a striking analogy Boscovich made clear to his own mind how small causes produce vast effects. He pictures a high mountain rising out of the sea, with sides so steep, that blocks of stone are just able to rest upon them without rolling down. He

supposes such blocks, diminishing gradually in size, to be strewn over the mountain—large below, moderate at the middle height, and dwindling to sand grains at the top. A small bird touches with its foot a grain on the summit;

Fig. 22.



it moves, sets the next larger grains in motion, these again let loose the pebbles, these the larger stones, these the blocks; until finally the whole mountain side rolls violently

into the sea, there producing mighty waves. Here the foot of the little bird unlocked the energy, the rest of the work being done by gravitation. This he regarded as an image whereby the propagation of fire might be rendered intelligible. The spark acts like the foot of the bird; it starts a process which is continued and vastly augmented by the molecular forces of the fuel. The force which moves a train is potential in the boiler before the steam is turned on. The hand of the engineer releases a detent and permits the potential to become actual. It, however, like the bird of Boscovich, only liberates a pre-existing power. The action of the nerves in unlocking the power of the muscles also falls in admirably with the conception of Boscovich here described.

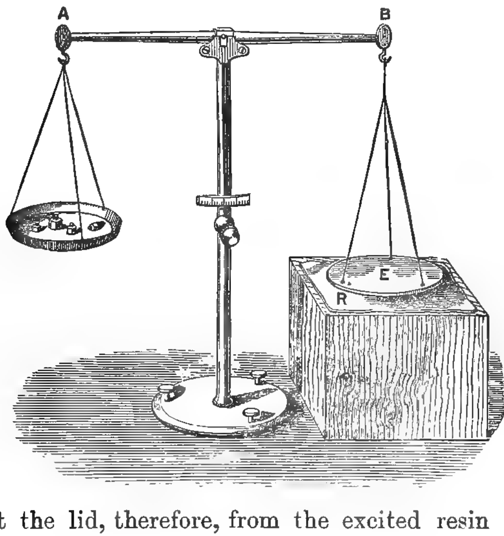
ELECTRICAL HEAT.

Electricity furnishes numerous illustrations of that principle of equivalence between the powers of Nature which it is the aim of these lectures to reveal. One such illustration was referred to when the discovery of Peltier was brought to bear in explanation of the phenomena of the thermo-pile. Further illustrations are to be now adduced. You are familiar with the action of the electrophorus. Before me is a large one, formed of a resinous cake, on which is placed a metal lid with an insulating handle. Removing the lid, I strike the resin with a piece of fur, or rub it with a piece of flannel, lay the lid upon it, touch the lid, lift it, and then bring my knuckle near its edge. I hear a crackle and see a spark. Whence come they? Heat, according to the principle of equivalence, requires for its production the expenditure of some other power. It is never spontaneously generated. To what expenditure, then, is our spark to be referred?

To answer this question, I extemporise a smaller electrophorus, using, instead of the resinous cake, a piece

of vulcanised india-rubber (R, fig. 23), and, instead of the large lid, a plate of brass, E, 5 inches in diameter. This plate is attached by silk threads, like a scale pan, to one end of a balance-beam (A B, fig. 23). It now rests upon the unexcited india-rubber. I determine its weight, and find it to be 100 grammes. Lifting the plate, I whisk the india-rubber briskly with a fox's brush, and bringing once more the plate down upon it, I touch the plate. To separate plate and rubber 115 grammes are now necessary.

FIG. 23.

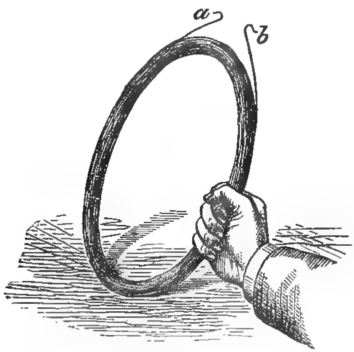


To lift the lid, therefore, from the excited resin cake a quantity of work has to be done in excess of what is necessary to overcome the mere force of gravity. The spark of the electrophorus is the exact equivalent of this excess. By-and-by we will trace farther back the pedigree of the spark. At present it suffices to point out its proximate parentage.

Similar considerations hold good for the electrical machine, from the conductor of which we obtain sparks of

considerable length and power. What is the force expended to produce them? The plate of the machine passes between two rubbers, ordinary mechanical friction coming here into play, and producing on the spot the heat due to such friction. Besides this, however, the electrical attraction between the rubber and the glass has to be overcome, and it is the particular portion of the force expended in overcoming this attraction that appears under the form of a spark. One great difference between electrical heat and frictional heat comes here into view; for, whereas the latter is developed at the place where the

FIG. 24.

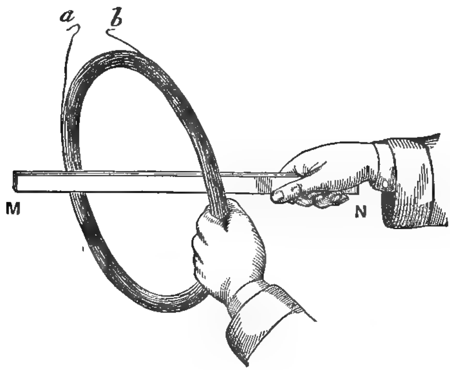


friction occurs, the former may be developed at any distance from its origin.

On the table before me lies a coil of wire, overspun with cotton, as it has come to us from the manufacturer. The two ends of the coil (a, b, fig. 24) are now unconnected. I lift one side of the coil, and in doing so overcome, in part, its gravity. Connecting its two ends, I lift it exactly as before. Were my 'muscular sense' fine enough, I should find that a greater amount of effort is here required to lift the coil than when gravity alone is overcome. What is the equivalent of this

excess of effort? It cannot be destroyed: what, then, has become of it? The answer is soon given. Instead of uniting the two ends of the coil directly, I connect them with a sensitive galvanometer. When the coil is lifted, we obtain immediately a deflection of the needle, proving that the lifting of the coil, when its ends are connected, evokes an electric current which passes round the circuit. This current is of momentary duration: it immediately resolves itself into heat which, in amount, is the exact equivalent of the excess of effort to which I have referred.

FIG. 25.



Holding the same coil, with its ends *a*, *b*, free, thus vertically (fig. 25), I thrust a bar-magnet, M N, horizontally half-way through it. An amount of force necessary to transport, with this velocity, a body of the weight of the magnet, is expended in this act; what is called 'inertia,' and it only, being overcome. I now connect together the two ends of the coil, and push the magnet once more through it. A greater amount of effort is here expended than before; and this excess of effort evokes an electric current, which instantly subsides, as heat, in the body of the coil. To withdraw the magnet, moreover,

a stronger pull is necessary when the coil is continuous than when it is broken, the augmentation of the pull finding its equivalent in an electric current, opposed in direction to the former one, but, like it, instantly resolving itself into heat.[1]

My muscles were not quite sensible of the excess of effort employed in these cases, but it is easy so to exalt the effect as to bring it within the range of consciousness. I pass a flat coil of wire, with its ends connected, between the poles of an unexcited electro-magnet, and encounter no sensible resistance. When the magnet is excited, the resistance to the passage of the coil is very sensible indeed. Suspending the coil so as to form a kind of pendulum, I draw it aside, and let it oscillate between the poles. When the magnet is unexcited, the oscillations continue for a considerable time. When the magnet is excited, the motion is instantly arrested. Bringing the two flat poles of the unexcited magnet near each other, I drop a half-crown between them. It falls as it would fall in ordinary air. When, however, the magnet is excited, the fall is visibly retarded. I have cut through this copper ring so as to break its continuity. I drop it between the excited poles, and determine the time which it requires to fall between them. Connecting the two ends of the ring, I repeat the experiment, and find the time of falling six times what it was before. The ring, in point of fact, drops as deliberately between the excited poles as if the space between them were filled with treacle or tar.

We are here dealing, as many of you know, with Faraday's induced currents, to which a circuit is opened by the completion of the ring. It is the interaction of the magnet and these currents which gives rise to the observed resistance to motion between the excited poles.

[1] A large lecture-room galvanometer suffices for this experiment.

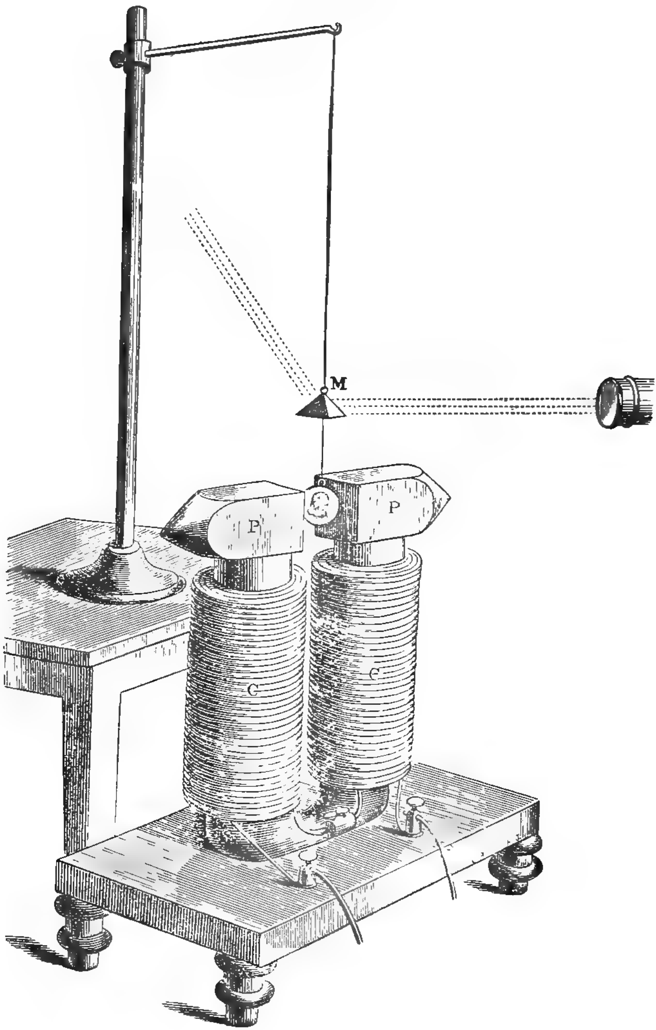
The final form of all such currents is heat, and this heat is the exact equivalent of the force expended in overcoming the magneto-electric attractions and repulsions.

With special apparatus constructed for the purpose, Faraday's currents may be raised to a great degree of intensity. Before you is a small machine presented to us by Mr. Wilde of Manchester, in which sixteen horse-shoe magnets are placed parallel to each other, an iron armature, with insulated copper wire coiled around it, being caused to rotate rapidly between their poles. With the ends of the coil unconnected, the amount of effort necessary to overcome its mechanical friction suffices to turn the machine. While turning it, I desire my assistant to connect the two ends of the coil. An increased amount of work is instantly thrown upon the muscles. If, prior to connecting the two ends of the coil, I happen to be turning the machine gently, on suddenly making the connection the rotation is as suddenly stopped. By applying more power I overcome the resistance, but, in doing so, develop an equivalent amount of heat in the coil. Instead of uniting the two ends of the coil by a thick copper wire, I connect them with a thin platinum one. A portion of the heat generated by my labouring muscle is made evident by the incandescence of the wire. I work the machine till the wire rises to a white heat, and continue working until it fuses. Instantaneous relief is afforded to my arm by the interruption of the contact between the two ends of the coil.

FUSION OF METAL IN THE MAGNETIC FIELD.

The foregoing considerations regarding electrical heat will prepare you for a striking experiment. We owe it in its first form to Dr. Joule; in its second form to the late M. Foucault; while the form in which it is now to

FIG. 26.



be introduced, is perhaps most suitable for the lecture room. This mass of iron—which is part of a link of a huge chain cable, surrounded by multiple coils of copper wire, C C (fig. 26)—can instantly be converted into a powerful magnet by sending an electric current through the wire. When thus excited, a poker clings to the iron, and chisels, screws, and nails cling to the poker. Turned upside down, this magnet will hold a half-hundredweight attached to each of its poles, and probably a score of the heaviest people in this room attached to the weights. At a proper signal the current is interrupted. The magnet now is mere common iron, which exerts no attractive power. On the ends of the magnet are placed two pieces of iron, P P—movable poles, as they are called—which can be brought within any required distance of each other. When the exciting current passes, these pieces of iron virtually form parts of the magnet. Between them I place a substance which the magnet, even when exerting its utmost power, is incompetent to attract. This substance is a piece of silver—in fact, a silver medal. When it is brought close to the excited magnet, no attraction ensues. Indeed what little force—and it is so little as to be utterly insensible in these experiments—the magnet really exerts upon the silver, is repulsive instead of attractive.

The medal now hangs between the poles. When a current is sent through the coil the silver is neither attracted nor repelled, but if we seek to move it we encounter resistance. To turn the medal round, this resistance must be overcome, the silver moving as if it were surrounded by a viscous fluid. This extraordinary effect may also be rendered manifest in another way. Causing a rectangular plate of copper to pass quickly to and fro like a saw between the poles P P, with their points turned towards each other; you seem, though you can see nothing, to be

sawing through cheese or butter.[1] No effect of this kind is noticed when the magnet is not active: the copper plate then encounters nothing but the infinitesimal resistance of the air.

Thus far you have been compelled to take my statements for granted, but an experiment is here arranged which will make this strange action of the magnet on the silver medal strikingly manifest to you all. Above the suspended medal, and attached to it by a bit of wire, is a little reflecting pyramid M, formed of four triangular pieces of looking-glass; both the medal and the reflector are suspended by a thread which was twisted in its manufacture, and which is caused to untwist itself by the weight of the medal. When a strong beam of light is thrown upon the little pyramid, the light is reflected, and, as the mirror turns, you see long luminous spokes moving through the dusty air of the room.

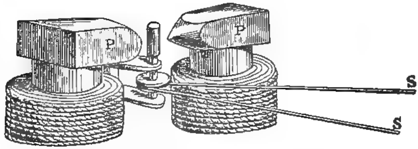
Let us start from a state of rest. The beam now passes through the room and strikes against the white wall. As the mirror begins to rotate, the patch of light moves, at first slowly, over the wall and ceiling. The motion quickens, the separate patches of light can no longer be seen, but instead of them we have a luminous band twenty feet in diameter, drawn upon the wall by the quick rotation of the reflected beams. At a signal given, the magnet is excited, the medal is struck motionless, the band suddenly disappears, and the single patch of light is again seen upon the wall. This strange result is produced without any visible change in the space between the two poles. Observe the slight motion of the image: the torsion of the string is struggling with an unseen antagonist and producing that motion. It is such as would be produced if the medal, instead of being surrounded by air,

[1] An experiment of Faraday's. He also was the first to arrest by a magnet the motion of a spinning cube of copper.

were immersed in a pot of treacle. On destroying the magnetic power, the viscous character of the space between the poles instantly disappears; the medal begins to twirl as before; producing the revolving beams and the luminous band.

By the force of the hand, as we have seen, the resistance between the excited poles can be overcome and the medal turned round. Force is thus expended—what becomes of that force? It is converted into heat. We have already referred to the discovery of Faraday, that electric currents are developed when a conductor of electricity is set in motion between the poles of a magnet. We have such currents here, and they are competent to heat the medal. But what *are* these currents? How are

FIG. 27.



they related to the space between the magnetic poles—how to the muscular force which is expended in their generation? It does not in the least lessen the interest of the experiment if the force of my arm, previous to appearing as heat, appears in another form—in the form of electricity. The result is the same: the heat developed ultimately is the exact equivalent of the power employed to move the medal in the excited magnetic field.

This development of heat is now to be made evident. For this purpose I use a solid metal cylinder, the core of which is composed of a metal more easily melted than its outer case. The outer case is of copper, and this is filled with a hard but fusible alloy. The cylinder is set upright between the conical poles P P (fig. 27) of the

magnet. A string s s passes from the cylinder to a whirling table, by which the cylinder may be caused to spin round. It might turn till doomsday with the magnet unexcited, and not produce the effect sought; but with the magnet in action an amount of heat will soon be developed sufficient to melt the fusible metal core. The cylinder is now rotating, its upper end being open. We will permit it to remain so until the liquid metal is seen spattering over the poles of the magnet. The metallic spray is already visible, though a minute has scarcely elapsed since the experiment began. I stop the motion for a moment, cork up the end of the cylinder, so as to prevent the loss of the metal, and let the action continue for half a minute longer. I withdraw the cylinder, remove the cork, and pour out before you the liquefied alloy.

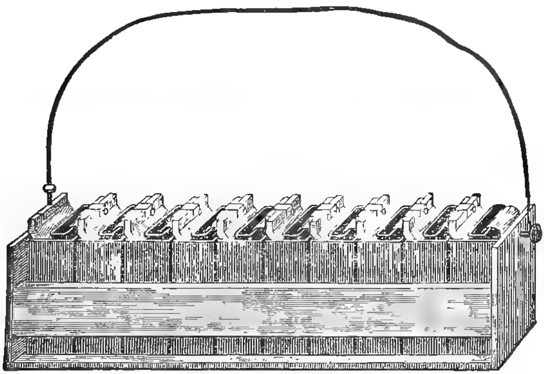
HEAT OF THE VOLTAIC BATTERY.

In Grove's voltaic battery, which is that commonly used in our experiments, zinc is dissolved—that is to say, burnt—in dilute sulphuric acid. By washing the clean surface of the zinc with mercury, we preserve it from solution until the electric current flows. As long as it continues to flow, the zinc is dissolved, sulphate of zinc being formed, which, when the action continues long enough, crystallises at the bottom of the cell. Now, the amount of heat generated by the combustion of an equivalent of zinc in the battery, is precisely the same as that generated in acidulated water outside the battery. And here I would ask you to bear in mind that, however the solution of the zinc may be carried on—whether the chemical action be feeble or intense, whether the solution be accomplished slowly or rapidly—the total amount of heat generated by the solution of a given quantity of zinc is invariable.

Two voltaic batteries of ten cells each are now before

you. I unite the two ends of one of them by a thick copper wire, as in fig. 28. Midway in the circuit of the other battery I introduce four inches of thin platinum wire; the currents flow through both circuits. I touch the thick copper wire, and find it cool: it is obvious to you that I dare not thus touch the platinum wire, for it is raised by the current to a white heat. It is not its thickness alone

FIG. 28.



that enables the copper wire to transmit the current without heating. Were it as thin as the platinum wire, though more warmed than it now is, it would fall far short of a red heat. The copper is what we call a good conductor, the platinum a bad conductor; the resistance of the latter being something tantamount to molecular friction, which the electric current has to overcome, and in overcoming which it develops heat. Or, the good conductor may be compared to a diathermanous body which permits of the free transmission of radiant heat, and remains cool; while the bad conductor may be compared to an athermanous body which stops the radiation, and may be raised even to incandescence by the heat intercepted.[1]

[1] The terms here employed are fully explained in a subsequent Lecture.

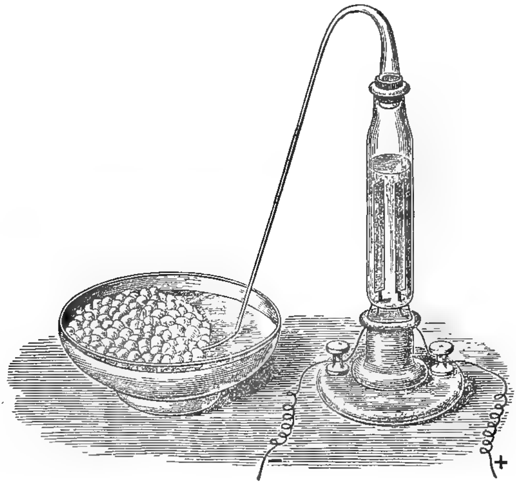
Permitting the two currents to flow until an ounce of zinc is dissolved in each battery, and starting from the principle that the solution of an ounce of zinc develops, under all circumstances, the same amount of heat, let us reason out what *must* occur in the case before us. Heat is generated in both batteries. But in the one case there is no heat outside, consequently it must be *all* within. In the other case part of the heat is generated outside; hence only a part of it can be within. But it is in the battery that the zinc is burnt; this last conclusion, therefore, implies that the heat can appear at a distance from the hearth where the fuel is consumed. If the heat generated in the cells which render the platinum wire incandescent were equal to the heat generated in those which produce no outside heat, then the heat of the platinum wire would be generated out of nothing. We should have a creation of heat; and this would be opposed to the principle of equivalence, in the pursuit and illustration of which we are now engaged. Thus far we reason and infer; let us, however, not trust to inference, but fall back upon experiment. Let us actually measure the heat. In the battery with the thick wire let the heat developed be represented by a large B, the heat of the other battery being represented by a small b; in the latter case we have superadded a quantity of external heat which we may call h. Experiment proves that

$$b + h = \mathrm{B};$$

in other words, the sum of the external and internal heats is a constant quantity. Thus while the absolute amount of heat generated by the oxidation of one ounce of zinc is, as already affirmed, invariable, it may be distributed in various proportions between the cells, and the circuit external to the cells. The sum of the external and internal heat never varies.

This principle of conservation cannot be eluded. Let us test it by another experiment. Water, you know, is decomposed by the voltaic current; and before you (fig. 29) is an apparatus which enables the decomposition to occur, and the resultant gases to be collected. Let our battery continue to decompose the water until, as before, an ounce of zinc has been consumed. The zinc in the battery fails to yield its due amount of heat, the missing quantity being applied to the work of external decomposition. But

FIG. 29.



take the gases resulting from that decomposition, and cause them, by ignition, to recombine; the exact amount of heat missing in the battery is thus restored.

We can here, in passing, solve an enigma which long perplexed scientific men. It is by power drawn from the battery that we are able to effect the decomposition of water outside the battery; but the maximum power of the battery is expressed by the amount of heat it can produce when no external heat is generated. Hence, if a demand

be made upon it greater than that which its maximum heat can supply, the demand cannot be responded to This, then, is the enigma just referred to. It was found that water could not be decomposed by a single cell, while it could be decomposed by two cells or by any greater number. Now, the law of electric decomposition is this: that for every equivalent of zinc consumed in the battery an equivalent of hydrogen is generated in the voltameter outside. But the equivalent of zinc in a single cell produces there 18,680 units of heat, while the combustion of an equivalent of hydrogen produces 34,460 units of heat. To decompose water, this latter amount of heat would have to be sacrificed; and it is obvious that a single cell which generates only 18,680 units is unable to meet this demand—in other words, a single cell is incompetent to decompose water. But when two cells are coupled together, we have 37,400 units developed by their joint action; and, this quantity of heat being in excess of that required for the decomposition of water, the two-cell battery is competent to effect the decomposition.[1]

A word or two may now be devoted to the electro-magnetic engine, a model of which, devised and constructed by that most able mechanician, the late M. Froment of Paris, is on the table. When a voltaic current is sent through the machine, in virtue of the electro-magnetic attractions and repulsions, we produce rapid rotation. This motion can be applied to pump water, to raise weights, and to do various other kinds of work. Let us start the machine itself and permit it to remain for a certain time in action. At the end of this time it is found warm, from the mechanical friction of its own parts and from other causes. The warmth thus observed has been withdrawn from the

[1] It is necessary in making this experiment that no current should have previously passed through the decomposition cell. For, aided by the 'polarisation' of the electrodes, a single cell can decompose water.

battery, the zinc dissolved during the time of rotation not having produced in the cells themselves its full equivalent of heat. And when we yoke the machine on to a pump and lift water, the force expended would, without this expenditure, have appeared as heat in the battery, which heat is accurately restored when the water falls again to the level from which it was raised. Thus all the effects derived from the magneto-electric engine are strictly compensated, this 'payment for results' being the inexorable method of nature. The excellent researches of M. Favre have thrown abundant light on all these questions.

MUSCULAR HEAT IN RELATION TO WORK.

No engine, however subtly devised, can evade this law of equivalence, or perform on its own account the smallest modicum of work. The machine distributes, but it cannot create. Is the animal body, which undoubtedly performs work, to be classed among machines? When you lift a weight, or throw a stone, or climb a mountain, or wrestle with a comrade, are you not conscious of actually creating and expending force? Let us look to the antecedents of this force. We derive the muscle and fat of our bodies from what we eat. Animal heat you know to be due to the slow combustion of this fuel. My arm is now inactive, and the ordinary slow combustion of my blood and tissue is going on. For every grain of fuel thus burnt a perfectly definite amount of heat has been produced. I now contract my biceps muscle without causing it to perform external work. The combustion is quickened, and the heat is increased; this additional heat being liberated in the muscle itself. I lay hold of a 56 lb. weight, and by the contraction of my biceps lift it through the vertical space of a foot. The blood and tissue consumed during this contraction have not developed in the muscle their due amount of heat. A quantity of heat

is at this moment missing in the muscle which would raise the temperature of an ounce of water somewhat more than 1° Fahr. I liberate the weight: it falls to the earth, and by its collision generates the missing heat. Muscular heat is thus transferred from its local hearth to external space. The fuel is consumed in the body, but the heat of combustion is produced outside the body. case is substantially the same as that of the Voltaic battery when it performs external work, or produces external heat.

We can do with our bodies all that we have already done with the battery—heat platinum wires, decompose water, magnetise iron, and deflect a magnetic needle. The combustion of our bodies may be made to produce all these effects, as the combustion of zinc may be caused to produce them. By turning the handle of a magneto-electric machine, a coil of wire was caused a few minutes ago to rotate between the poles of a magnet. As long as the two ends of the coil were unconnected we had simply to overcome the ordinary inertia and friction of the machine in turning the handle. But the moment the two ends of the coil were united by a thin platinum wire a sudden addition of labour was thrown upon the turning arm. When the necessary labour was expended, its equivalent immediately appeared. The platinum wire glowed; it was maintained at a white heat, and finally fused. From the muscles of the arm, with a temperature of 100°, we extracted the temperature of molten platinum, which is nearly 4000°. The similarity of the action with that of the Voltaic battery when it heats an external wire is too obvious to need pointing out. When the machine is used to decompose water, the virtual heat of the muscle, like that of the battery, is consumed in molecular work, being fully restored when the gases recombine.

The matter of the human body is the same as that of the world around us; and here we find the forces of the human body identical with those of inorganic nature. Just as little as the Voltaic battery is the animal body a creator of force. It is an apparatus exquisite and effectual beyond all others in transforming and distributing the energy with which it is supplied, but it possesses no creative power.[1]

[1] The foregoing paragraphs are extracted from 'Fragments of Science,' 6th edition, vol. ii. p. 348.

LECTURE IV.

CHANGES OF VOLUME PRODUCED BY HEAT—EXPANSION OF SOLIDS—THEORETIC EXPLANATIONS OF EXPANSION—THE TREVELYAN INSTRUMENT—CONTRACTION OF STRETCHED INDIA-RUBBER BY HEAT—EXPANSION OF LIQUIDS—MAXIMUM DENSITY OF WATER—CONTRACTION BY HEAT AND EXPANSION BY COLD—FORCE OF CRYSTALLISATION—BURSTING OF IRON ENVELOPES—CONSEQUENCE OF DEPORTMENT OF WATER IN NATURE—ERROR OF RUMFORD'S SPECULATIONS—EXPANSION OF BISMUTH IN CRYSTALLISING—THE MERCURIAL THERMOMETER.

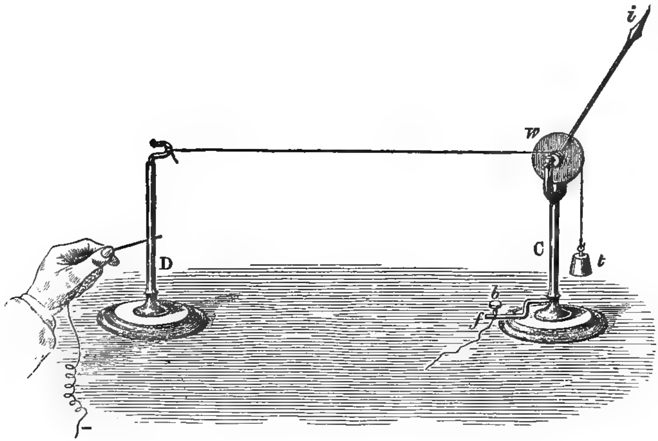
APPENDIX:—FURTHER REMARKS ON DILATATION.

EXPANSION OF SOLIDS BY HEAT.

THERE are almost innumerable ways of illustrating the expansion of solids by heat. A closely-fitting cylinder which passes through a round hole when cold, cannot be forced through it when heated. A cold bar which fits between the two sides of a gauge will not fit when heated. One or two simple illustrations of the fact of expansion will here suffice. Between two stout upright rods of copper, C and D (fig. 30), stretches a thin platinum wire. Over the axis of the little wheel, *w*, which has a grooved edge, slides a narrow tube which permits the wheel to rotate freely. One end of the platinum wire is coiled round the narrow tube. A small weight, dependent from a string which passes over the grooved edge of the wheel, keeps the wire gently stretched. The wheel is accompanied in its motions by the index, *i*, which is simply a long, light straw. If the wire be tightened, the index moves in one direction; if it be relaxed, the index moves in the opposite direction. The binding-screw, *b*, is permanently

attached to one end of a voltaic battery, while a wire from the other end is held in the hand. Touching the rod, D,

FIG. 30.



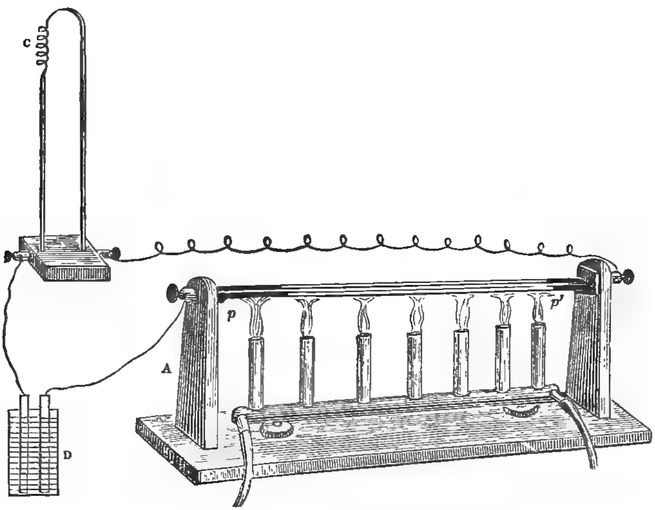
for a moment with this free wire, the battery current passes through the platinum wire; it is heated, and its expansion is instantly declared by the fall of the index. On breaking the circuit the wire cools, contracts, and the index returns to its first position. A momentary touch again brings the index down. The current, though convenient, is not necessary; the flame of a spirit lamp passed along the wire produces the same effect.

Again, here are two wooden stands, A and B (fig. 31), with plates of brass riveted against them. Two bars, $p\,p'$, of equal length, one of them brass, the other iron, are not, as you observe, sufficiently long to stretch from plate to plate. They are therefore supported on two little projections of wood attached to the stands. The plate of brass to the left is connected with one pole of a voltaic battery, D, while from the other plate a wire proceeds to the little instrument in front of the table; and thence to the other

pole of the battery. The instrument in front consists merely of an arrangement to support a spiral, C, of platinum wire, which will glow with a pure white light when the current from D passes through it.

At the present moment the only break in the circuit is due to the insufficient length of the bars of brass and iron to bridge the space from stand to stand. Underneath the bars is a row of gas jets, which I will now ignite; the

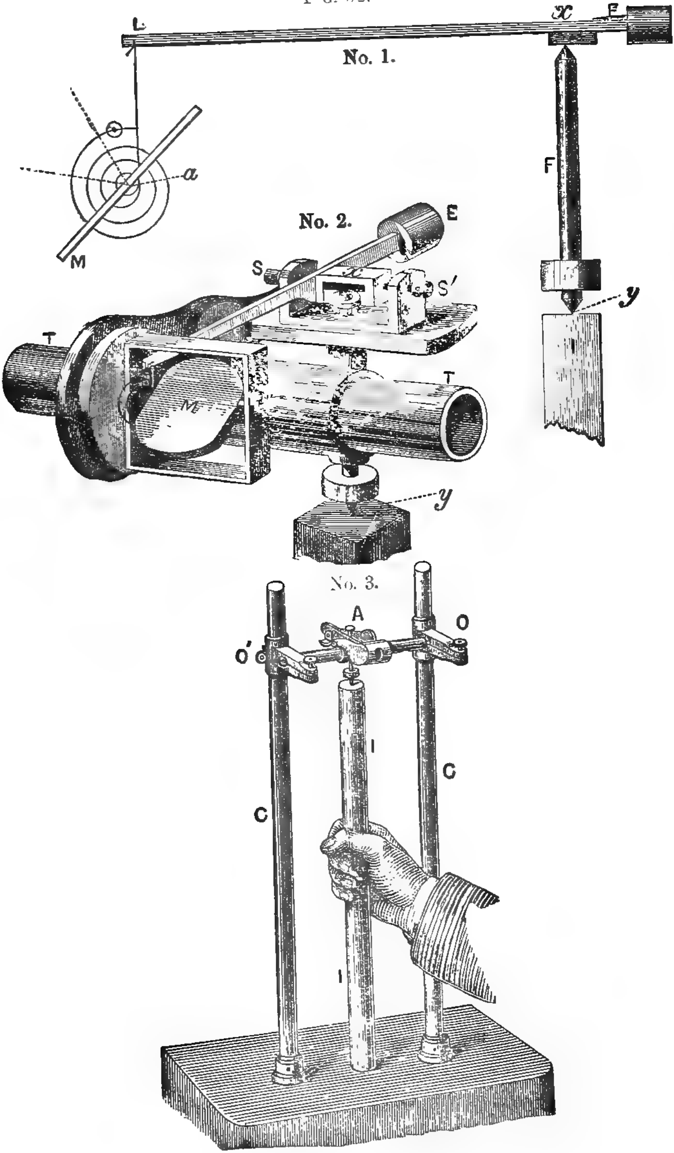
FIG. 31.



bars are heated and the metals expand. The spiral remains for a time non-luminous, the bridge not being complete; but now it brightens up, proving the extension of one or both the bars from stand to stand. On removing the iron, the platinum still glows: restoring the iron, and removing the brass, the light disappears. It was the brass, then, that bridged the gap. So that we have here an illustration, not only of the general fact of expansion, but also of the fact that different bodies expand in different degrees.

Less with the view of further illustrating this subject than of bringing to your notice the devices employed by scientific men to make small quantities sensible, I ask your attention to the apparatus in front of the table. Into a solid block of wood (No. 3, fig. 32, next page) are firmly fixed two cylindrical brass pillars, C C, 1 inch in diameter and 35 inches high. Over the pillars pass two clamps, O O′, and from one clamp to the other stretches a cylindrical cross-bar 11 inches long and $\frac{3}{4}$ of an inch wide. This bar is capable of two motions; the first up and down, the second round its own axis. To this cross-piece is attached an apparatus A, intended to magnify the vertical expansion or contraction of the lead bar, I I. On the top of this bar rests one end of a small cylindrical brass rod with pointed steel ends. This rod fits accurately into a brass collar (partially seen in No. 2), moving up and down in the collar with the least possible friction. The other point of the rod presses against a plate of agate very close to a pivot round which the plate can turn. The agate plate is attached to a brass lever of the third order 2·1 inches long, the fulcrum of which is the pivot just mentioned. Any motion of the agate against which the steel point presses is magnified about fifty times at the end of the lever. From this end a piece of fine steel wire passes round the axis of a rotating mirror, which turns when the end of the lever moves. This magnifying apparatus is shown on a larger scale in No. 2, where M is the mirror, S and S′ two centre-screws whose points constitute the pivot round which the lever turns. E is a small counter-weight, T T is the cross-piece to which the magnifying apparatus is attached. A naked section of the apparatus is also given in No. 1, where I is the lead bar, F the brass rod with the pointed steel ends, divested of its collar; one of its ends pressing against the plate of agate near the pivot, x, and the other

FIG. 32.



end resting upon the top of the lead bar at y. From the end, L, of the lever the steel fibre passes round the axis, a, of the mirror M. Thus the effect is multiplied, first by the lever, and then by the angular motion of the mirror.

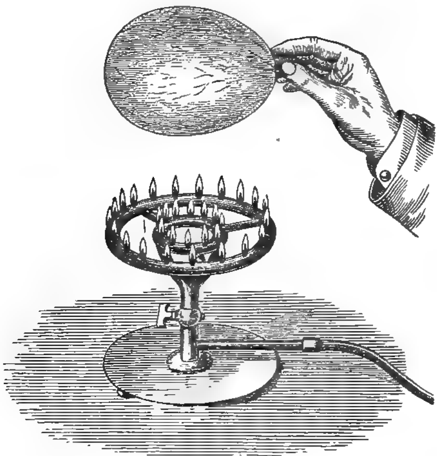
The beam from our electric lamp now falls upon the mirror, and is reflected as a luminous disk to the top of the screen. Breathing against the bar produces a sensible motion of the disk. Clasping the bar carefully with the hand, the warmth communicated brings the disk down through a vertical space of 20 feet. Projecting against the bar a fillet of warm water from a syringe-bottle, the action is very prompt and energetic. Projecting against it a fillet of alcohol, the vaporisation of that liquid chills the bar, and the index returns from the floor to the ceiling. This instrument was devised for showing the change of length of an iron or bismuth bar by the act of magnetisation; but applied as here described, it is converted into a thermoscope of exceeding delicacy.[1]

While illustrating experimentally the facts of expansion by heat, we are in the region of ordinary experience; but there is something within us which prevents us from resting there. What, we ask, is the internal mechanism by which expansion is effected? Here, again, we must help ourselves to conceptions of the invisible by reference to the visible. An experiment will make the matter clear. Over a ring burner (fig. 33), and at some distance above the flame, I hold a bladder containing but a little air. Turning it briskly round so as to avoid scorching, all parts of the bladder are heated by the ascending current. The air within the bladder shares the heat of its envelope: it swells in consequence; and now the bladder, which a moment ago was flaccid, is tightly stretched. In a way usual to the human mind the expan-

[1] By an inadvertence the bar in No. 1 and No. 2 is shown square.

sion of atmospheric air, thus illustrated, was transferred from the world of the senses to the region of atoms and molecules. These were supposed to be surrounded by atmospheres of caloric; and it was simply the expansion of these atmospheres which pushed the atoms apart, and produced the observed dilatation. Such was the explanation of dilatation by heat given by the material theory.

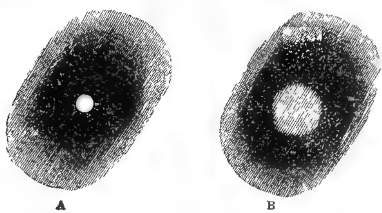
FIG. 33.



But how are we to picture such dilatation in accordance with the theory which regards heat as a mode of motion? The comparison of a very great thing with an indefinitely small one will here help us to a clear conception. I once approached Gibraltar on a fine star-light night when the planet Jupiter was sharply defined on a clear sky (A, fig. 34). On walking, however, past the funnel of the steamer, so as to bring the heated air between me and it, the planet suddenly augmented in apparent size, losing at the same time part of its sharpness of definition (B, fig. 34). The expansion was evidently due to the heated air, causing the image of the planet to quiver on

the retina. This quivering was in all directions, and it was so rapid that the various motions blended upon the retina to a disc of augmented size. If, instead of the planet's light being acted upon by heated air, the planet itself had danced in all directions to and fro, the same apparent augmentation of the disc would have ensued. Jupiter, thus quivering, would virtually fill a greater space than if he were still. The case is similar with our dancing atoms. When, instead of a motionless atom, we have a vibrating one, we must make room not only for the atom itself, but also for the distance over which its motion stretches The case may be further illustrated by a tuning-fork Motionless as it is at present, its prongs fit into a certain

FIG. 34.



space; thrown into vibration, the prongs strike against their boundaries, demanding more room. Throwing the shadow of a large vibrating fork upon a screen, the augmentation due to width of swing is rendered clearly visible, the prongs, like the planet, losing at the same time a portion of their definition.

The range of this atomic vibration differs in different bodies. In lead and zinc, for example, for the same increase of temperature, it is far greater than in iron. In brass, also, which is an alloy of zinc and copper, it is greater than in iron. Hence, as we have seen, brass, on being heated, expands more than iron. The architect and engineer have to exercise care in combining together bodies

of different expansibilities.[1] Of these two rulers, for example, one is brass and the other iron, and they are riveted together so as to form, at this temperature, a straight compound ruler. But when the temperature is changed, the ruler is no longer straight. If heated, it bends in one direction; if cooled, it bends in the opposite direction. When heated, the brass expands most, and forms the convex side of the curved ruler. When cooled, the brass contracts most, and forms the concave side of the ruler. Facts like these must, of course, be taken into account in structures where it is necessary to avoid distortion. The force with which bodies expand when heated, and contract when cooled, is practically irresistible. All these molecular forces, indeed, though operating in such minute spaces, are almost infinite in energy. The contractile force of cooling has been applied by engineers to draw leaning walls into an upright position.

Before you are some flasks of very thick glass—Bologna phials as they are called—which, when blown, were allowed to cool quickly. The external portions became first chilled and rigid. The internal portions cooled more gradually; but they found themselves, on cooling, surrounded, as it were, by a rigid shell, on which they exerted the powerful strain of their contraction. The consequence is, that the superficial portions of these flasks are in such a state of tension that the slightest scratch produces rupture. The mere dropping of a little bit of hard quartz into a flask causes the bottom to fly out of it. Here also are these so-called Rupert drops, or Dutch tears, produced by glass being fused to drops, and suddenly cooled. The external rigid shell has to bear the strain of the inner contraction; but the strain is distributed so equally all over the surface, that no part gives

[1] The coefficients of expansion of a few well-known substances are given in the Appendix to this Lecture.

way. But by simply breaking the filament of glass, which forms the tail of the drop, the solid mass explodes to powder. I dip a drop into a thin medicine bottle filled with water, and break the tail outside; the drop is shivered with such force that the shock, transferred through the water, is sufficient to break the bottle in pieces.[1]

A very curious effect of expansion was observed, and explained, some years ago, by the late Canon Moseley. The choir of Bristol Cathedral was covered with sheet lead, the length of the covering being 60 feet, and its depth 19 feet 4 inches. It had been laid on in the year 1851, and two years afterwards it had moved bodily down through a distance of eighteen inches. The descent had been continually going on from the time the lead had been laid down, and an attempt made to stop it by driving nails into the rafters had failed; for the force of descent was sufficient to draw out the nails. The roof was not a steep one, and the lead would have rested on it for ever, without *sliding*. What, then, was the cause of the descent? Simply this. The lead was exposed to the varying temperatures of day and night. During the day the heat imparted to it caused it to expand. Had it lain upon a horizontal surface, it would have expanded equally all round; but as it lay upon an inclined surface, it expanded more freely downwards than upwards. When, on the contrary, the lead contracted at night, its upper edge was drawn more easily downwards than its lower edge upwards. Its motion was therefore that of a common earthworm; it pushed its lower edge forward during the day, and drew its upper edge after it during the night, and thus by degrees it crawled through a space of eighteen inches in two years. Every minor change of temperature during the day and during the night contributed also to

[1] This known principle has been recently applied to the explosion of bombshells. It has probably an important future.

the result; indeed Canon Moseley afterwards found the main effect to be due to these quicker alternations of temperature.

Not only do different bodies expand differently by heat, but the same body may expand differently in different directions. In crystals, the atoms are so laid together that along some lines they are more closely packed than along others. It is also likely that the atoms of many crystalline bodies oscillate more freely and widely in some directions than in others. The consequence of this would be an unequal expansion by heat in different directions. Iceland spar was proved by Mitscherlich to expand more along its crystallographic axis than in any other direction. Nay, while the crystal expands as a whole —that is to say, while its volume is augmented by heat— it actually contracts on being heated, in a direction at angles to the crystallographic axis. Many other crystals also expand differently in different directions; and, I doubt not, most organised structures would, if examined, exhibit the same fact.

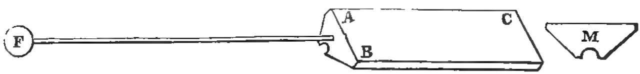
THE TREVELYAN INSTRUMENT.

Before finally quitting the expansion of solids, I wish to show you an experiment which illustrates in a curious and agreeable way the conversion of heat into mechanical energy. The fact to be reproduced was first observed by Schwartz, in one of the smelting works of Saxony. A quantity of silver which had been fused in a ladle was left to solidify, and to hasten its cooling it was turned out upon an anvil. Some time afterwards a strange buzzing sound was heard in the locality. The sound was finally traced to the hot silver, which was found quivering upon the anvil. Many years subsequent to this, Mr. Arthur Trevelyan chanced to be using a hot soldering-iron, which he laid by accident

against a piece of lead. Soon afterwards, his attention was excited by a most singular sound, which, after some searching, was found to proceed from the soldering-iron. Like the silver of Schwartz, the soldering-iron was in a state of vibration. Mr. Trevelyan made his discovery the subject of a very interesting investigation. He determined the best form to be given to the 'rocker,' as the vibrating mass is now called; and throughout Europe this instrument is known as 'the Trevelyan Instrument.' These curious vibrations and tones have engaged the attention of Principal J. D. Forbes, Dr. Seebeck, Mr. Faraday, M. Sondhaus, and myself; but to Trevelyan and Seebeck most of our knowledge regarding the subject is to be ascribed.

Before you is a brass rocker (fig. 35), whose length, A C, is five inches; the width, A B, 1·5 inch; and the

FIG. 35.

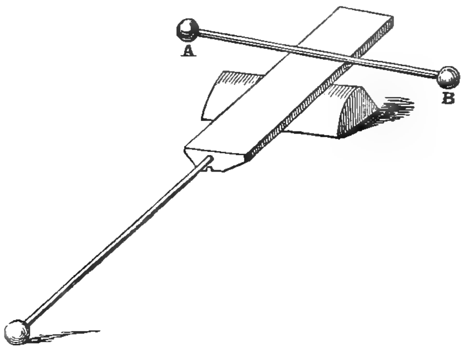


length of the handle, which terminates in the knob F, ten inches. Along the back of the rocker runs a groove which is shown in the cross-section given at M. Heating the rocker to a temperature not sufficient to fuse lead, we lay it on a block of that metal, allowing its knob to rest upon the table. You hear a quick succession of forcible taps; but you cannot see the oscillations of the rocker, to which the taps are due. I therefore place on it a brass rod A B (fig. 36), with a ball at each end; the oscillations are thereby rendered much slower, and you can easily follow with the eye the pendulous motion of the balls. This motion will continue as long as the rocker is able to communicate sufficient heat to the carrier on which it rests. The vibrations can be rendered quicker

by using a rocker with a wider groove. Placing such a rocker upon the lead, as before, it fills the room with a clear full note. Its taps are periodic and regular, and so rapid that they have linked themselves together to produce music.

These singular vibrations and tones are an effect of expansion. Whenever the hot metal comes into contact

Fig. 36.



with its lead carrier, a nipple suddenly juts from the latter, being produced by the heat communicated to the lead at the point of contact. The rocker is thus tilted up. Some other point of it comes immediately into contact with the lead, a fresh nipple is formed, and the rocker is again tilted. Let A B (fig. 37) be the surface of

Fig. 37.

the lead carrier, and R the cross-section of the hot rocker. Tilted to the right, the nipple is formed as at R; tilted to the left, it is formed as at L, the nipple in each case disappearing as soon as the contact with the rocker ceases. The consequence is, that while its temperature remains

sufficiently high, the rocker is tossed to and fro, and the quick succession of its taps against the lead produces a musical sound.

In the Trevelyan experiment the great point is to secure *local* expansion, which requires certain conditions to be fulfilled. The rocker ought to be a good conductor, and yield its heat freely to the mass underneath. Brass fairly fulfils this condition. The carrier ought to have a high coefficient of expansion; it ought also to be an imperfect conductor; for otherwise the heat, instead of being concentrated at the point of contact, and suddenly producing a nipple there, would be diffused throughout the mass. Lead fulfils both these conditions. Zinc, though possessing as high a coefficient of expansion as lead, does not make a good rocker, mainly because of its high 'capacity' for heat. Equal quantities of heat communicated to equal weights of lead and zinc make the increase of temperature of the former three times that of the latter. This question of capacity shall be fully discussed subsequently. What has been here stated shows the variety of considerations which come into play, in thoroughly disentangling what might be considered a very simple physical problem.

The localisation of the heat may be effected by varying the shape of the carrier. A brass *block* will not answer, but two brass pins placed upright in a vice will cause a rocker to oscillate. By devices of this kind, as shown by Seebeck, all solid metals may be rendered effective as carriers. The inclusion of minerals such as rock-salt, rock-crystal, fluor-spar, chalcedony, &c., in the list of carriers was effected by myself. A very pretty experiment by Mr. George Gore, wherein a light metal ball is caused to roll along heated metal rails, is to be explained in the same manner as the vibrations of the Trevelyan rocker.

Looked at with reference to the connection of natural forces, the Trevelyan experiment is not without interest. The atoms of bodies must be regarded as all but infinitely small, but then they must be regarded as all but infinitely numerous. The augmentation of the amplitude of any oscillating atom by the communication of heat may be insensible; but the summation of an almost infinite number of such augmentations becomes sensible. Such a summation, effected almost in an instant, produces the nipple, and tilts the heavy mass of the rocker. Here we have a direct conversion of heat into common mechanical motion. The nipple is neither as hot nor as high as it would be if it had not lifted the rocker. The tilted rocker falls again by gravity, and in its collision with the block restores the precise amount of heat which was consumed in lifting it. Here we have the conversion of gravitating force into heat. Again, the rocker is surrounded by the air of this room, which weighs some tons, every particle of which, and every tympanic membrane, and every auditory nerve present, is shaken by the rocker. Thus we have the conversion of a portion of the heat into sound. Finally, every sonorous vibration which speeds through the air and wastes itself upon the walls, seats, and cushions of this room, is converted into the form with which the cycle of actions began—namely, into heat.

CONTRACTION OF STRETCHED INDIA-RUBBER BY HEAT.

Nature is full of anomalies which no foresight can predict, and which experiment alone can reveal. From the deportment of a vast number of bodies, we should be led to conclude that heat always produces expansion, and that cold always produces contraction. We have now to notice a first exception to this general rule. If a metal be compressed, heat is developed; but if a wire be stretched, cold is the result. Dr. Joule and others have worked ex-

perimentally at this subject, and found this fact all but general. One striking exception to the rule (there are probably many others) has been known for a great number of years. The sheet of india-rubber now handed to me has been placed in the next room to keep it quite cold. Cutting from this sheet a strip three inches long, and an inch and a half wide, and turning our thermo-pile upon its back, I lay upon its exposed face the strip of india-rubber. The deflection of the needle proves that the rubber is cold. Laying hold of the ends of the strip, I suddenly stretch it, and press it, while stretched, on the face of the pile. The needle moves with energy, showing that the stretched rubber has heated the pile.

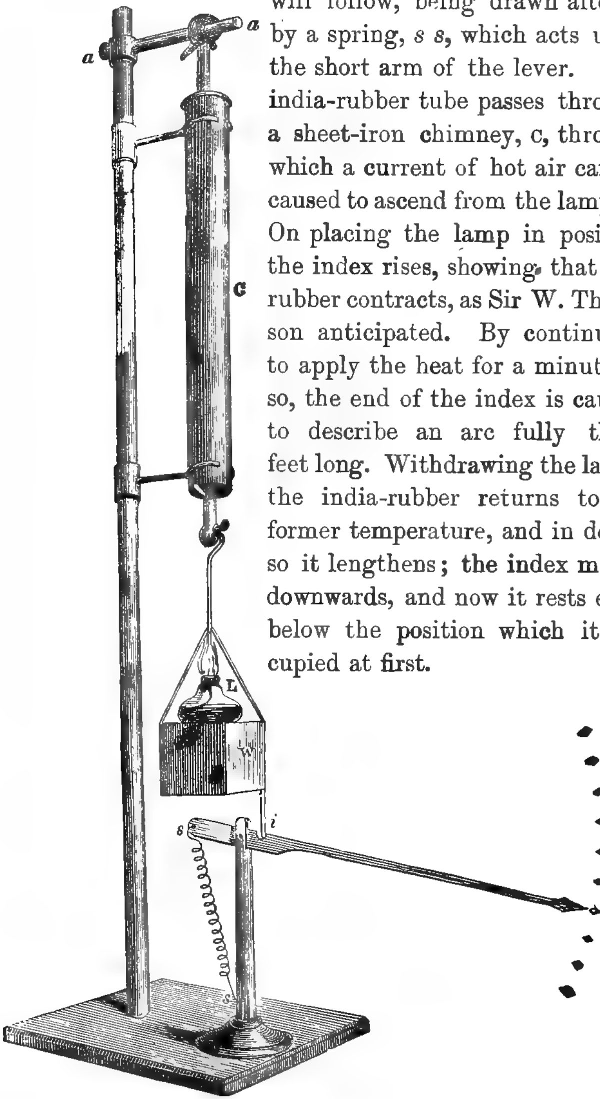
But one deviation from a rule always carries other deviations in its train. In the physical world, as in the moral, acts are never isolated. In many of his investigations Dr. Joule has been associated with Sir William Thomson, who, when made aware of the deviation of india-rubber from an almost general rule, suggested on theoretic grounds that the stretched india-rubber might *shorten*, on being heated. The test was applied by Joule, and the shortening was found to take place.[1] This singular experiment, thrown into a suitable form, is now to be made in your presence.

To the horizontal arm, *a a* (fig. 38), is fastened a length of common vulcanised india-rubber tubing, stretched by a weight, W, of ten pounds, to about three times its normal length. The index, *i i*, is formed first of a piece of light wood moving freely on a pivot, being prolonged by a stout straight straw. At the end of the straw is placed a spear-shaped piece of paper, which can range over a graduated circle. The index is now pressed down at *i*, by a projection attached to the

[1] Phil. Mag. 1857, vol. xiv. p. 227.

weight. If the weight should be lifted by the contraction of the india-rubber, the index will follow, being drawn after it by a spring, *s s*, which acts upon the short arm of the lever. The india-rubber tube passes through a sheet-iron chimney, C, through which a current of hot air can be caused to ascend from the lamp, L. On placing the lamp in position the index rises, showing that the rubber contracts, as Sir W. Thomson anticipated. By continuing to apply the heat for a minute or so, the end of the index is caused to describe an arc fully three feet long. Withdrawing the lamp, the india-rubber returns to its former temperature, and in doing so it lengthens; the index moves downwards, and now it rests even below the position which it occupied at first.

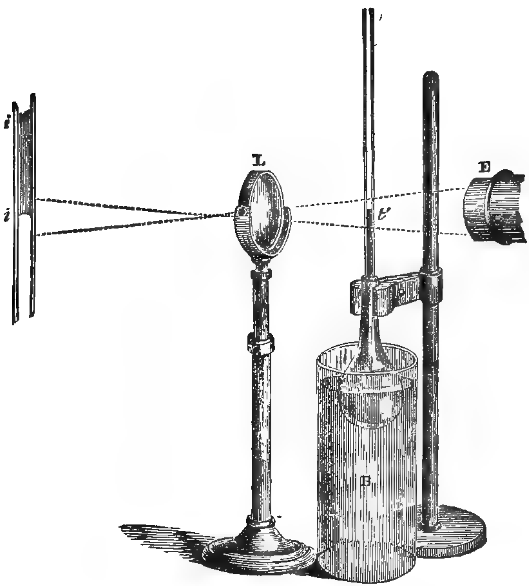
FIG. 38.



EXPANSION OF LIQUIDS BY HEAT—EXCEPTIONS.

To illustrate the expansion of liquids by heat we take a Florence flask filled with alcohol, and tightly corked. Through the cork a tube, *t t′* (fig. 39), passes water-tight, the liquid standing at some height above the

FIG. 39.



cork in the tube. When the liquid in the flask is heated it will expand and rise in the tube. To enable you to see it rising, the tube *t t′* is placed in front of the electric lamp E, a strong beam of light being sent across it where the liquid column ends. In front of the tube is placed a lens L, which casts an enlarged image *i i* of the column upon the screen. It is needless to say that the image is inverted, and that when the alcohol expands, the top of the column

will *descend* along the screen. I might apply a flame to heat the alcohol, but dearly-bought experience warns me of the danger of doing so, for the cracking of the flask would be followed by a combustion very difficult to quench. I therefore fill a beaker, B, with hot water, and raise the beaker so that the hot water shall surround the alcohol. For a moment the head of the column *ascends*, as if the liquid contracted on the application of the heat. But in a moment this motion ceases, descent begins, and it will continue permanently. But why the first ascent? It is not due to the contraction of the liquid, but to the momentary expansion of the flask, to which the heat is first communicated. The glass expands before the heat can fairly reach the liquid, and hence the column falls; but the expansion of the liquid soon exceeds that of the glass, and the column rises. We are here taught that the observed dilatation of the liquid does not give us its true augmentation of volume, but only the difference of dilatation between it and the glass.

With another flask exactly equal in size to the former, but filled with water, I repeat the experiment made with the alcohol. You notice, first of all, the transitory effect due to the expansion of the glass, and afterwards the permanent expansion of the liquid; but you observe that the dilatation proceeds much more slowly than in the case of alcohol. Liquids more volatile than alcohol expand still more rapidly. The expansion of liquid carbonic acid, for example, far exceeds that of alcohol. Now we might examine a hundred liquids in this way, and find them all expanding by heat, and we might thus be led to conclude that expansion by heat is a law without exception; but we should err in this conclusion. It is really to illustrate an exception of this kind that this flask of water has been introduced.

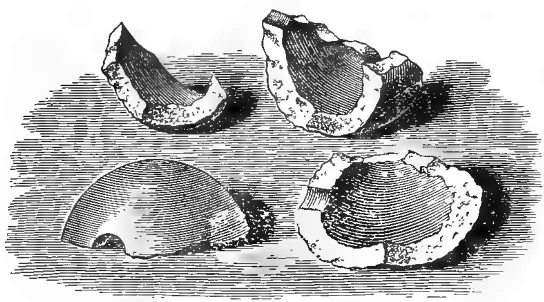
I will now throw this experiment, which, although rarely

made in lectures, is one of great physical interest, into a form which will enable you to repeat it with certainty. For practical reasons I abandon the vessel B, fig. 39, and heat the flask containing the water with the flame of a spirit lamp. The column rises, and we will permit it to rise till it reaches the top, *t*, of the tube and trickles over. It now does so before your eyes. I next transfer the flask to a basin, and rapidly surround it with a mixture of pounded ice and salt. The liquid column immediately begins to sink, because of the contraction of the water in the flask. Give the experiment your patience. The sinking of the column continues for a time, but it becomes more and more slow, and finally it ceases altogether. The column halts motionless for a brief interval, and now it is visibly rising. The cold here acts the part played by the heat a few minutes ago. The liquid column gradually approaches, and at length attains the top of the tube; and now the water trickles over as before. The experiment is an impressive one. If heat be now applied to our flask, the action is immediately reversed; the column descends, showing the contraction of the water by heat. After a time contraction ceases, and permanent expansion sets in. Here, then, we have Nature pausing in her ordinary course, and reversing her ordinary habits. The fact is, that the water goes on contracting till it reaches a temperature of 39° Fahr., or 4° Cent., at which point the contraction ceases. This is the *point of maximum density* of water; from this downwards, to its freezing point, the liquid expands; and when it is converted into ice, the expansion is sudden and considerable. Ice, we know, swims upon water, being lightened by this expansion.

The force with which water expands in freezing is all but irresistible. With the view of giving you an illustration of this fact, water has been confined in this iron

bottle which is fully half an inch thick; the quantity of water being small, though sufficient to fill the bottle. The bottle is closed by a screw firmly fixed in its neck. Two bottles thus prepared are placed in a copper vessel, and surrounded with a freezing mixture. They cool gradually, the water within them approaching its point of maximum density. No doubt, at this moment, a small vacuous space exists within each bottle. But soon the contraction ceases, and expansion sets in. The vacuous space is slowly filled, the water gradually changes from liquid to solid. To accomplish this change it requires

FIG. 40.



more room, which the rigid iron refuses to grant. But its rigidity is powerless in the presence of these molecular forces, and the sound you now hear indicates that the bottle is shivered by the crystallising molecules. The other bottle follows; and here are the fragments of the vessels, showing their thickness, and impressing you with the vastness of the expansive force by which they have been thus riven.[1] While I have been speaking, you have heard a louder explosion in front of the table. That was due to the rupture of a thick bombshell kindly prepared for

[1] Metal cylinders, an inch in thickness, are unable to resist the decomposing force of a small galvanic battery. M. Gassiot has burst many such cylinders by electrolytic gas.

me at Woolwich by Professor Abel. It was filled with water, screwed up tight, placed in a bucket and surrounded by a freezing mixture. Taken from the mixture the fragments of the bomb are placed here before you (fig. 40). Care must be taken in repeating this experiment to cover the bucket with a thick cloth. Wanting such protection I have seen the stopper of a broken bomb projected nearly as high as this ceiling.

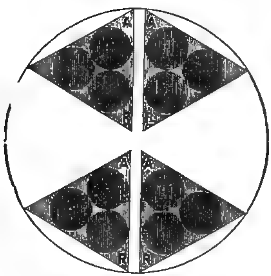
You have now no difficulty in understanding the effect of frosty weather upon the water-pipes of your houses. Before you are some pieces of such pipes, all rent. You become first sensible of the damage when the thaw sets in, but the mischief is really done at the time of freezing; the pipes are then burst, and through the rents the water escapes, when the ice liquefies.

Let us endeavour to obtain a mental image, even if it be only an approximation to the truth, of the processes and power here illustrated. First, then, it is to be noted that the change from contraction to expansion by diminution of temperature does not occur until the water approaches the point where it forsakes the liquid, and assumes the solid crystalline form. The ice-crystal occupies a volume larger by one-eighth than the water from which it is produced; hence it is that ice swims upon the water as the lighter body. Now the enlargement in the case of the crystal can only be due to the rearrangement of the water molecules; and this rearrangement can, in its turn, only be due to molecular forces which come first sensibly into play at a low temperature. Like minute magnets the molecules are gifted with mutually attractive and repellent poles, the action of which is insensible until they have been drawn by diminution of temperature sufficiently near each other. The temperature 39° Fahrenheit marks the point where the tendency of the molecules, as wholes, to approach each other, is exactly neutralised by the anta-

gonistic action of their poles; while from 39° to 32° the polar forces are more and more predominant, their triumph culminating in the act of solidification. The whole process, then, of expansion from 39° to 32° is to be regarded as incipient crystallisation, which ends in the locking together of the poles at the lower temperature.

Our conceptions here may be helped by a model which will show how an augmentation of volume *may* result from molecular polar action. The molecule of water is composed of three atoms—one of oxygen and two of hydrogen. Let the shaded triangles in the adjacent figure (fig. 41) represent the spaces taken up by the molecules. Suppose the

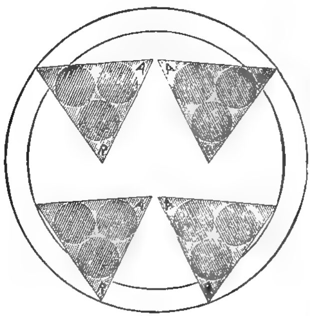
FIG. 41.



points marked A to be mutually attractive, and those marked R mutually repellent, and that the position of the triangles as shown in the figure corresponds to the maximum density of water. Then the retreat of the poles R from each other, and the approach of the poles A towards each other, causing each molecule to rotate, will produce an encroachment of the molecules upon the circumjacent space. This is shown in an exaggerated form in fig. 42. It is some such encroachment as that here rudely figured as a *possible* molecular action, which our bombshell proved unable to withstand.

It is hardly necessary for me to say a word on the importance of this property of water in the economy of nature. Rumford was so impressed with it that he devoted a whole chapter to speculations regarding it. 'It does not appear to me,' he writes, 'that there is anything which human sagacity can fathom, within the wide-extended bounds of the visible creation, which affords a more striking or more palpable proof of the wisdom of the Creator, and of the special care He has taken in the general arrangement of the universe to preserve animal life, than this

FIG. 42.



wonderful contrivance.' Rumford's enthusiasm was excited by considerations like the following: Suppose a lake exposed to a clear wintry sky. The superficial water is first chilled; it contracts, becomes heavier, and sinks by its superior weight, its place being taken by the lighter water from below. In time this is chilled and sinks in its turn. Thus a circulation is established, the cold dense water descending, and the lighter and warmer water rising to the top. Supposing this to continue, even after the first pellicles of ice have been formed at the surface; the ice would sink, and the process would not cease until the entire water of the lake would be solidified. Death to every living thing in

the water would be the consequence. But just when matters become critical, Nature, speaking poetically, steps aside from her ordinary proceeding, causes the water to expand by cooling, and the cold water to swim like a scum on the surface. Solidification ensues, but the solid is much lighter than the subjacent liquid, and the ice forms a protecting roof over the living things below.

Rumford obviously regarded this behaviour of water as a solitary exception to the general laws of nature. 'Had not Providence,' he says, 'interfered on this occasion in a manner which may well be considered as *miraculous*,' the solitary reign of eternal frost would have spread on every side from the poles. 'In latitudes where now the return of Spring is hailed by the voice of gladness, where the earth decks herself in her gayest attire, and millions of living beings pour forth their songs of joy and gladness, nothing would have been heard but the whistling of the rude winds, and nothing seen but ice and snow, and flying clouds charged with wintry tempests.' He begs the reader's candour and indulgence while he investigates the subject. 'I feel,' he says, 'the danger to which a mortal exposes himself who has the temerity to undertake to explain the designs of Infinite Wisdom.' But though he admits the enterprise to be adventurous, he contends that it cannot be improper.

Facts like those discussed by Rumford naturally and rightly excite the emotions. Indeed, the relations of life to the conditions of life—the general adaptations of means to ends in Nature—excite, in the profoundest degree, the interest of the philosopher. But in dealing with natural phenomena, the feelings must be carefully watched. They often lead us unconsciously to overstep the bounds of real knowledge, and to run into generalisations which are in perpetual danger of being overthrown. Give place to the emotions by all means; they belong to

the forces of nature; but let them be wisely guided and securely based. Let not the vine of feeling twine itself round a decaying stem, lest the fall of the stem should endanger the life of the vine. Rumford was wrong in supposing that the case of water illustrated a miraculous interposition of Providence; for the case is not an isolated one. Before you is an iron bottle rent from neck to bottom; and when it is broken with a hammer you see a core of metal within. The metal is bismuth, which was poured when molten into this bottle and confined there by a tightly fitting screw, exactly as the water was confined. The metal cooled, solidified, expanded, and the force of expansion sufficed to rend the bottle. There is no life here to be saved, still the bismuth accurately imitates the behaviour of water. Once for all, it may be said that the natural philosopher, as such, has nothing to do with purposes and designs. His vocation is to inquire *what* Nature is, not *why* she is; though he, like others, and he more than others, must stand at times rapt in wonder at the mystery in which he dwells, and towards the final solution of which his studies fail to furnish him with a clue.

THE MERCURIAL THERMOMETER.

The mercurial thermometer presents an important illustration of the expansion of liquids by heat. Water owes its liquidity to molecular motion; when this motion subsides sufficiently, crystallisation, as we have seen, sets in. The temperature of crystallisation is perfectly constant, if the water be kept under the same pressure. The temperature of condensation from the state of steam is also constant, as long as the pressure remains the same. Here, then, we have two invariable standard points of temperature, and they have been used as such throughout

the world. The mercurial thermometer consists of a bulb and a stem with capillary bore. The bore ought to be of equal diameter throughout. The bulb and a portion of the stem are filled with mercury. Both are then plunged into melting ice, the mercury shrinks, the column descends, and finally comes to rest. Let the point at which it becomes stationary be marked; it is the *freezing point* of the thermometer. The instrument is next plunged into boiling water, or rather into the steam above boiling water; the mercury expands, the column rises, and finally attains a stationary height. Let this point be marked; it is the *boiling point* of the thermometer. The space between the freezing point and the boiling point has been divided by Réaumur into 80 equal parts, by Fahrenheit into 180 equal parts, and by Celsius into 100 equal parts, called degrees. The thermometer of Celsius is also called the Centigrade thermometer.

Both Réaumur and Celsius call the freezing point 0°; Fahrenheit calls it 32°, because he started from a zero which he erroneously imagined was the greatest terrestrial cold. Fahrenheit's boiling point is therefore 212°, Réaumur's boiling point is 80°, while the boiling point of Celsius is 100°.

The length of the degrees being in the proportion of 80 : 100 : 180, or of 4 : 5 : 9, nothing can be easier than to convert one into the other. If it be required to convert Fahrenheit into Celsius, we multiply by 5 and divide by 9; if Celsius into Fahrenheit, we multiply by 9 and divide by 5. Thus 20° of Celsius are equal to 36° Fahrenheit; but if we would know what temperature by Fahrenheit's thermometer corresponds to 20° of Celsius, we must add 32 to the 36, which would make the temperature 20°, as shown by Celsius, equal the temperature 68°, as shown by Fahrenheit.

APPENDIX TO LECTURE IV.

FURTHER REMARKS ON DILATATION.

It is not within the scope of the present work to dwell in detail on all the phenomena of expansion by heat; but, for the sake of my younger readers, I will supplement this chapter by a few additional remarks.

The linear, superficial, or cubic coefficient of expansion, is that fraction of a body's length, surface, or volume which it expands on being heated one degree.

Supposing one of the sides of a square plate of metal, whose length is 1, to expand, on being heated one degree, by the quantity a, the linear coefficient of expansion; then the side of the new square is $1+a$, and its area is

$$1+2a+a^2.$$

In the case of expansion by heat, the quantity a is so small that its square is almost insensible; the square of a small fraction is, of course, greatly less than the fraction itself. Hence, without sensible error, we may throw away the a^2 in the above expression, and then we have the area of the new square

$$1+2a.$$

$2a$, then, is the superficial coefficient of expansion; hence we infer that by multiplying the linear coefficient by 2, we obtain the superficial coefficient.

Suppose, instead of a square, that we had a cube, having a side $=1$; and that on heating the cube one degree, the side expanded to $1+a$; then the volume of the expanded cube would be

$$1+3a+3a^2+a^3.$$

In this, as in the former case, the square of a, and much more the cube of a, may be neglected, on account of their exceeding smallness; we then have the volume of the expanded cube

$$=1+3a;$$

that is to say, the cubic coefficient of expansion is found by trebling the linear coefficient.

The following table contains the coefficients of expansion for a number of well-known substances:

Copper . . .	0·000017	0·000051	0·000051
Lead . . .	0·000029	0·000087	0·000089
Tin . . .	0 000023	0·000069	0·000069
Iron . . .	0·0000123	0·000037	0·000037
Zinc . . .	0 0000294	0·000088	0·000089
Glass . . .	0·000008	0·000024	0·000024

The first column of figures gives the linear coefficient of expansion for 1° C.; the second column contains this coefficient trebled, which ought, if the foregoing statements be correct, to be the cubic expansion of the substance. This is checked by the third column, which gives the cubic expansion as determined directly by Professor Kopp. It will be seen that Kopp's coefficients agree almost exactly with those obtained by the trebling of the linear coefficients.

The linear coefficient of glass for 1° C. is

0·0000080.

That of platinum is

0·0000088.

Hence glass and platinum expand nearly alike. This is of the greatest importance to chemists, who often find it necessary to *fuse* platinum wire into their glass tubes. Were the coefficients different, the fracture of the glass would be inevitable during the unequal contraction.

LECTURE V.

THE SOLID, LIQUID, AND GASEOUS FORMS OF MATTER—KINETIC THEORY OF GASES—COEFFICIENT OF EXPANSION—ITS CONSTANCY IN THE CASE OF GASES—GASES HEATED UNDER CONSTANT PRESSURE AND AT CONSTANT VOLUME—ABSORPTION OF HEAT IN WORK—MAYER'S CALCULATION OF THE MECHANICAL EQUIVALENT OF HEAT—JOULE'S EXPERIMENTAL DETERNATION OF MECHANICAL EQUIVALENT—DILATATION OF GASES WITHOUT REFRIGERATION—ABSOLUTE ZERO OF TEMPERATURE—LIQUEFACTION OF GASES, INCLUDING OXYGEN, HYDROGEN, AND AIR.

ON the occasion of our first meeting here a sledge-hammer was permitted to descend upon a lump of lead, which was heated by the blow. Formerly it was assumed that the force of the hammer was simply lost by the concussion. In elastic bodies it was supposed that a portion of the force was restored by the rebound; but in the collision of inelastic bodies it was taken for granted that the force of impact was lost. We now admit no loss, but assume, that when the motion of the descending hammer ceases, it is simply a case of transference, instead of annihilation The motion of a mass has been transformed into molecular motion. Here the imagination must help us. In the case of solid bodies, while the force of cohesion still holds them together, you must conceive a power of vibration, within certain limits, to be possessed by their atoms. And the greater the amount of heat imparted to the body, or the greater the amount of mechanical action invested in it by percussion, compres-

sion, or friction, the greater will be the rapidity of some, and the wider the amplitude of other, atomic oscillations.

As already indicated, the atoms or molecules thus vibrating, and ever as it were seeking wider room, urge each other apart, and thus cause the body of which they are the constituents to expand in volume. By the force of cohesion, then, the molecules are held together; by the force of heat they are pushed asunder; and on the relation of these two antagonistic powers the aggregation of the body depends. Every fresh increment of heat pushes the molecules more widely apart; but the force of cohesion, like all other known forces, acts more and more feebly as the distance through which it acts is augmented. As, therefore, the heat grows strong, its opponent grows weak, until, finally, the particles are so far loosened from the thrall of cohesion, as to be at liberty, not only to vibrate to and fro across a fixed position, but also to roll or glide around each other. Cohesion is not yet destroyed, but it is so far modified that the particles, while still offering resistance to being torn directly asunder, have their lateral mobility over each other's surfaces secured. *This is the liquid condition of matter.*

In the interior of a mass of liquid, the motion of every molecule is controlled by the molecules which surround it. But when we develop heat of sufficient power, even within the body of a liquid, the molecules break the last fetters of cohesion, and fly asunder to form bubbles of vapour. If, moreover, one of the surfaces of the liquid be quite free, that is to say, uncontrolled either by a liquid or a solid, it is easy to conceive that some of the vibrating superficial molecules will be jerked entirely away from the liquid, and will fly with a certain velocity through space. *Thus freed from the influence of cohesion, we have matter in the vaporous or gaseous form.*

This conception of gaseous molecules is now generally accepted, as expressing the truth of nature. Such molecules are supposed to be always flying in straight lines through space. The hypothesis has been developed in our day by Joule, Krönig, and Maxwell, but chiefly in a series of admirable papers by Clausius. The quickness with which the perfume of an odorous body fills a room, might seem to harmonise with the idea of direct projection. It may, however, be proved, that if the theory of rectilinear motion be true, the molecules must move at the rate of several hundred feet a second. Hence it might be objected that, according to the above hypothesis, odours ought to spread much more rapidly than they are observed to do.

The answer to this objection is, that the odoriferous molecules have to make their way through a crowd of air atoms, with which they come into incessant collision. On an average, the distance through which such a molecule can travel without striking against an atom of air, is infinitesimal, the propagation of a perfume through air being thus enormously retarded by the air itself. When a free communication is opened between the surface of a liquid and a vacuum, the vacuous space is almost instantaneously filled with the vapour of the liquid.

It is not difficult to determine the average velocities with which, according to this hypothesis, the atoms of various gases move. Taking, for example, a gas at the pressure of an atmosphere, and placing it in a vessel a cubic inch in size and shape, we can calculate from the weight of the gas the velocity with which its particles must strike each side of the cube in order to counteract a pressure of 15 lbs. The lighter the gas is, the greater of course must be its velocity to produce the required effect. According to Clausius (Phil. Mag., 1857, vol. xiv. p. 124), the average velocities of the atoms of oxygen,

nitrogen, and hydrogen, at the temperature of melting ice, are respectively as follows:

Oxygen	1,514 feet per second.		
Nitrogen	1,616	„	„
Hydrogen	6,050	„	„

As far back as 1848, Mr. Joule found the velocity of hydrogen atoms to be 6,055 feet per second.

According to this theory, which is known as the Kinetic Theory of gases, we are to figure the molecules of a gas as flying in straight lines through space, impinging like little projectiles upon each other, and striking against the boundaries of the space they occupy. I place a bladder, half filled with air, under the receiver of the air-pump, and remove the air from the receiver. The bladder swells. According to our present theory, this expansion of the bladder is produced by the shooting of atomic projectiles against its interior surface. When air is admitted into the receiver, the bladder shrivels to its former size; and here we must figure the discharge of the atoms against the outer surface of the bladder, driving the envelope inwards, causing, at the same time, the atoms within to concentrate their fire, until finally the force from within equals that from without, and the envelope remains quiescent. All the impressions, then, which we derive from heated air or vapour are, according to this hypothesis, due to the impact of gaseous molecules. Thus the impression one receives on entering the hot room of a Turkish bath, is caused by the atomic patter there maintained against the surface of the body.

When, instead of placing the bladder under the receiver of an air-pump, and withdrawing the external air, we augment, by heat, as in a former experiment, the projectile force of the atoms within it, these strike with such impetuous energy against the inner surface as to cause

the envelope to retreat: the bladder swells and becomes apparently filled with air.

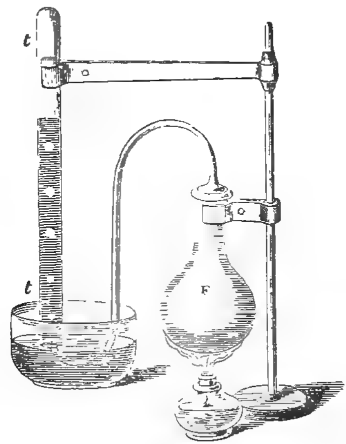
Correct ideas, like sound seed, require a soil, that is to say, the soil of public intelligence, to render them fruitful. Wanting this they often perish, or exist for long periods in a state of suspended animation. In more favourable times other minds arise, and, from the contemplation of a wider basis of fact, arrive at and render permanent the same ideas. This might seem to be only an act of revival, but it is often a real re-creation. This, in my opinion, has been the case in regard to the mechanical theory of heat as a whole, and also in regard to the special part of that theory on which we are now engaged. The kinetic theory of gases which has been just described was enunciated by Daniel Bernouilli in 1738. He considered the case of a vertical cylinder containing very small molecules and closed by a movable lid on which was placed a weight. The molecules he supposed to be darting in all directions with enormous velocity. Striking against the lid they would, he alleged, support it exactly like an elastic fluid which, when the weight is diminished, expands, and when the weight is augmented contracts in volume. Such an assemblage of flying molecules could, according to Bernouilli, produce all the physical effects which had been actually observed with air, and they also suggested and explained other effects which had not yet been investigated.

'If,' he says, 'the weight on the lid be augmented, and the gas compressed, the lid has to endure on the part of the fluid a resistance increased twofold. First, because the number of the molecules in relation to the space they occupy is rendered greater, and secondly because each molecule repeats its shock more frequently than before. The nearer the molecules are pressed together, the more

rapidly must their shocks succeed each other. But the elasticity of air is not only increased by compression, it is also increased by heat. And, as it is assumed that the heat is always augmented when the motion of the molecules is increased, it follows that the augmented elasticity of warmed air, at a constant volume, arises from a more violent motion of the molecules.'

We have here an arrangement intended to show in a simple manner the expansion of gases by heat. The flask, F (fig. 43), is empty, except as regards air, which

FIG. 43.



may be heated by placing a spirit-lamp underneath the flask. From the flask a bent tube passes to a dish, containing a coloured liquid. In the dish, a glass tube, *t t*, two feet long, closed at the top and full of the coloured liquid, is inverted. The liquid column is sustained by the pressure of the atmosphere. The tube passing from the flask F is caused to turn up exactly underneath the open end of the upright tube, so that if a bubble of air should

issue from the former, it will ascend the latter. I now heat the flask; the air expands, and bubbles are driven from the end of the bent tube. They ascend in the tube *t t*, and depress the coloured liquid, until in the course of a very few seconds the whole column of liquid has been displaced by air.

The general fact of expansion is thus simply illustrated, but we must not be content with regarding these phenomena in a general way. Without exact quantitative determinations our discoveries would soon confound and bewilder us. We must now inquire what is the amount of expansion which a given quantity of heat is able to produce in a gas? This is an important point, and demands our special attention. In speaking of the volume of a gas, we should have no distinct notion of its real value if the temperature of the gas were omitted, so largely does the volume vary with the temperature. Place, then, a definite measure of gas at the precise temperature of water when it begins to freeze, or of ice when it begins to melt, that is to say, at a temperature of 32° Fahr. or 0° Cent., in an envelope which offers no resistance to expansion. Raise that measure of gas one degree in temperature, *the atmospheric pressure on the unresisting envelope which holds the gas being preserved perfectly constant.* The gas will expand by a quantity which we may call a; raise it another degree in temperature, its volume will be expanded by $2a$, a third degree will cause an expansion of $3a$, and so on. For every degree added to the temperature of the gas, it expands by the same amount. What is this amount? No matter what the volume of the gas may be at the freezing temperature, the addition of one degree *Fahrenheit* to that temperature augments its volume by $\frac{1}{490}$ of its own amount; while by raising it one degree *Centigrade* we augment the volume by $\frac{1}{273}$ of its own amount. A cubic foot of gas, for example,

at 0° C. becomes, on being heated to 1°, $1\frac{1}{273}$ cubic foot, or, expressed in decimals—

1 vol. at 0° C. becomes 1 + ·00366 at 1° C.;
at 2° C. becomes 1 + ·00366 × 2;
at 3° C. becomes 1 + ·00366 × 3, and so on.

The constant number ·00366, which expresses the fraction of its own volume, which a gas, at the freezing temperature expands on being heated one degree, is called the *coefficient of expansion* of the gas. Of course if we use the degrees of Fahrenheit, the coefficient will be smaller in the proportion of 9 to 5.

It is a significant fact that all the so-called permanent gases expand by almost precisely the same amount for every degree added to their temperature. We can deduce from this with extreme probability the important conclusion, that where heat causes a true gas to expand, the work it performs consists solely in overcoming the external pressure —that, in other words, the heat is not interfered with by the mutual attraction of the gaseous molecules. For if this were the case, we should have every reason to expect, in the case of different gases, differences of expansion similar to those observed in liquids and solids. I said intentionally 'by *almost* precisely the same amount,' for many gases which seem permanent at ordinary temperatures deviate slightly from the rule. This will be seen from the following table:

Name of Gas	Coefficient of Expansion
Hydrogen	0·00366
Air	0·00367
Carbonic oxide	0·00367
Carbonic acid	0·00371
Protoxide of nitrogen	0·00372
Sulphurous acid	0·00390

Here hydrogen, air, and carbonic oxide agree very closely; still there *is* a slight difference, the coefficient for

hydrogen being the least. In the other cases we remark a greater deviation from the rule; and it is particularly to be noticed that the gases which deviate most are those *which are nearest their point of liquefaction.* Until the end of 1877 the first three gases in the table resisted all attempts to liquefy them, while the others had yielded to the combined action of cold and pressure. These latter, therefore, were considered to be *imperfect* gases, occupying a kind of intermediate place between the liquid and the perfect gaseous condition.

MAYER'S CALCULATION OF THE MECHANICAL EQUIVALENT OF HEAT.

This much made clear, we shall now approach, by slow degrees, a difficult, but most important, subject—none indeed more important, or involving more momentous issues, in the whole range of physical science. That a definite relation existed between the heat developed by mechanical action and the force which produces it, floated doubtless in many minds before it received either correct enunciation or experimental proof. The celebrated Montgolfier entertained the idea of the equivalence of heat and work; and the idea was developed by his nephew, M. Séguin, in his volume 'On the Influence of Railways,' printed in 1839. Those, moreover, who reflect on the vital processes—thermal, chemical, and mechanical—which occur in the animal body, and on the relation of all of them to the force of food, are led naturally to entertain the idea of interdependence between forces in general. It is therefore hardly a matter of surprise that the man who was the first to raise this idea to a previously unattained clearness in his own mind, was a physician. In 1842, the late Dr. Mayer, of Heilbronn in Germany, briefly enunciated the relation which subsists between the forces of inorganic

nature, winding up his reflections by the determination of the 'mechanical equivalent of heat.'[1] He followed up, as will be shown in due time, the exposition of the principle of equivalence by its fearless application. But the theoretic views of Mayer, profound and far-reaching as they were, required an experimental basis of a strength commensurate with their importance; and to Dr. Joule, of Manchester, belongs the honour of building this irrefragable foundation.[2] We shall take the labours of these two eminent men in their historic order, clearing the way for Mayer by a brief development of the data on which he founded his calculation of the mechanical equivalent of heat.

Suppose a quantity of air to be contained in a very tall cylinder, A B (fig. 44), the transverse section of which is one square inch in area. Let the top A of the cylinder be open to the air, and let P be a piston, which can move air-tight and *without friction* up or down in the cylinder. For reasons to be explained immediately, I will suppose the piston to weigh two pounds one ounce. At the commencement of the experiment let the piston be at the middle point P of the cylinder, and let the distance from B to P be 273 inches—the air underneath the piston being

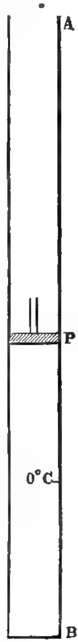
[1] Liebig's Annalen, vol. xlii. p. 233; Phil. Mag. 4th Series, vol. xxiv. p. 371; and in *résumé*, Phil. Mag. vol. xxv. p. 378.

I have been indebted to Sir C. Wheatstone for the perusal of a rare and curious pamphlet by G. Rebenstein, with the following (translated) title: 'Progress of our Time. Generation of Heat without Fuel; or, Description of a Mechanical Process, based on physical and mathematical proofs, by which Caloric may be extracted from Atmospheric Air, and in a high degree concentrated. The cheapest Substitute for Fuel in most cases where combustion is necessary.' Rebenstein deduces from the experiments of Dulong the quantity of heat evolved in the compression of a gas. No glimpse of the dynamical theory is, however, to be found in his paper; his heat is *matter* (*Wärmestoff*) which is squeezed out of the air as water is out of a sponge.

[2] Mr. Joule's experiments on the mechanical equivalent of heat extend from 1843 to 1849.

at a temperature of 0° C. Then, on heating the air from 0° to 1° C., the piston, as we have already learned, will rise one inch, and stand at 274 inches above the bottom. If the temperature be raised two degrees, the piston will stand at 275; if raised three degrees, it will stand at 276; if raised ten degrees, it will stand at 283; if 100 degrees, it will stand at 373 inches above the bottom. Finally, if the temperature were raised to 273° C., it is quite manifest that 273 inches would be added to the height of the column, or, in other words, that by heating the air to 273° C., *its volume would be doubled.*

FIG. 44.



In this experiment, the expanding air executes work. In lifting the piston from P to A it overcomes the downward pressure of the atmosphere, which amounts to 15 lbs., and also the weight of the piston, which is 2 lbs. 1 oz. The work done by the air is, therefore, equivalent to the raising a weight of 17 lbs. 1 oz., or 273 ounces, to a height of 273 inches. The same amount of work would be accomplished if the atmosphere above P were entirely abolished, a frictionless piston weighing 17 lbs. 1 oz. being placed at P.

Let us now alter our mode of experiment, and instead of allowing the air to expand, let us oppose its expansion by augmenting the pressure upon it. In other words, let us keep *its volume constant* while it is being heated. Suppose, as before, the initial temperature of the gas to be 0° C., the pressure upon it, including the weight of the piston P, being as formerly 273 ounces. Let us warm the gas from 0° C. to 1° C.; what weight must we add at P in order to keep its volume constant? Exactly one ounce. But we have supposed the gas, at the commencement, to be under a pressure of 273 ounces, and the pressure it

sustains is the measure of its elastic force ; hence, by being heated one degree, the elastic force of the gas has augmented by $\frac{1}{273}$ of what it was at 0°. If we warm it 2°, 2 ounces must be added to keep its volume constant: if 3°, 3 ounces must be added. And if we raise its temperature 273°, we shall have to add 273 ounces, or, in other words, we must *double the original pressure*, to keep the volume constant.

It is simply for the sake of clearness, and to avoid fractions, that I have supposed the air to be under the original pressure of 273 ounces. For as long as the air behaves as a sensibly perfect gas, no matter what the pressure may be, the addition of 1° C. to its temperature produces an augmentation of $\frac{1}{273}$ of the elastic force which the air possesses at 0° C., while by raising its temperature 273° without expansion, its elastic force is doubled. Let us now compare this experiment with the last one. *There* we heated a certain amount of gas from 0° to 273° C. and doubled its volume by so doing, the double volume being attained by lifting a weight of 273 ounces through a height of 273 inches. *Here* we heat the same amount of gas from 0° to 273°, but we do not permit it to lift any weight. The quantity of matter heated in both cases is the same ; the temperature to which it is heated is the same; but are the absolute quantities of heat imparted in both cases the same? By no means. Supposing that to raise the temperature of the air, whose *volume* is kept constant, 273°, the heat of 10 grains of burning wax is necessary; then to raise the temperature of the air, whose *pressure* is kept constant, an equal number of degrees, would require the consumption of 14¼ grains of the same combustible matter. At this point the genius of Mayer struck in. The material theory had referred the extra consumption to the greater 'capacity' of the rarefied air for heat. Mayer, on the contrary, discarding the un-

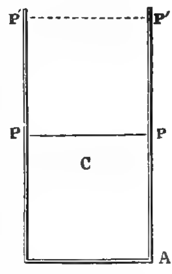
proved and incorrect assumption of changed capacity, regarded *the heat of the additional* $4\frac{1}{4}$ *grains of wax as entirely consumed in lifting the weight.* Thus the material theory supposed the heat; though hidden, to be still in existence, as heat; while Mayer's theory affirmed the total disappearance of the heat through its expenditure in work. Using accurate numbers, the quantity of heat applied when the pressure is constant, is to the quantity applied when the volume is constant, as

$$1{\cdot}421 : 1.$$

The quantity of work here executed and the quantity of heat expended are both perfectly definite; hence the possibility of comparing them together, and of expressing the one in terms of the other. In this establishment of an exact quantitative relation between Heat and Work, the speculations, reasonings, and experiments of all the philosophers mentioned in our Second Lecture find their culmination. I will now endeavour to calculate before you the Mechanical Equivalent of Heat.

Let C (fig. 45) be a cylindrical vessel with a base one square foot in area. Let P P mark the upper surface of a cubic foot of air at a temperature of 0° C. or 32° Fahr. The height A P will then be one foot. Let the air be heated till its volume is doubled. To effect this it must, as before explained, be raised 273° C., or 490° F. in temperature; and when expanded, its upper surface will stand at P′ P′, one foot above its initial position. But in rising from P P to P′ P′ it has forced back the atmosphere, which exerts a pressure of 15 lbs. on every square inch of its upper surface, the area of which is 144 square inches. In other words, it has lifted a weight of 144 × 15 = 2,160 lbs. to a height of one foot.

FIG. 45.



The usual way of expressing numerically a definite quantity of heat is to state the number of pounds of water which it could raise 1° in temperature. The 'unit of heat' is the quantity which would raise 1 lb. of water 1°. My aim now is to express in such units the quantity of heat applied in the foregoing experiment to the performance of work. Comparing equal weights of air and water, the quantity of heat required to raise the temperature of the former one degree would raise that of the latter a little less than a quarter of a degree. Employing the old phraseology, the 'capacity' of water for heat being 1, the capacity of air would be a little less than $\frac{1}{4}$. Strictly speaking, it would be 0·24. Now the weight of our cubic foot of air is 1·29 oz.; hence the quantity of heat required to raise 1·29 oz. of air 490° Fahr. would raise a little less than one-fourth of that weight of water 490°. The exact quantity of water equivalent to our 1·29 oz. of air is $1{\cdot}29 \times 0{\cdot}24 = 0{\cdot}31$ oz.

But 0·31 oz. of water, heated to 490°, is equivalent to 152 ozs. or $9\frac{1}{2}$ lbs. heated 1°. Thus the heat imparted to our cubic foot of air, in order to double its volume, and enable it to lift a weight of 2,160 lbs. one foot high, would be competent to raise $9\frac{1}{2}$ lbs. of water one degree in temperature.

The air has here been heated under the constant pressure of the atmosphere, and we have learned that the quantity of heat expended on air under constant pressure is to that expended on the same air at constant volume as 1·421 : 1; hence we have the statement:

$$1{\cdot}421 : 1 = \overset{\text{lbs.}}{9{\cdot}5} : \overset{\text{lbs.}}{6{\cdot}7},$$

which shows that the quantity of heat necessary to augment the temperature of our cubic foot of air, at constant volume, 490°, would raise the temperature of 6·7 lbs. of water 1° F.

Deducting 6·7 lbs. from 9·5 lbs., we find that the excess of heat imparted to the air, in the case where it is permitted to expand, is competent to raise 2·8 lbs. of water 1° in temperature.

As explained already, this excess is employed to lift a weight of 2,160 lbs. one foot high. Dividing 2,160 by 2·8, we find that a quantity of heat sufficient to raise 1 lb. of water 1° Fahr. in temperature, is competent to raise a weight of 771·4 lbs. a foot high. If the centigrade scale be used the equivalent is 1390 foot-pounds.[1]

This is Mayer's calculation of the Mechanical Equivalent of Heat.

Mayer's first brief paper, which was published in the spring of 1842, contains merely an indication of the way in which he had found the equivalent. In that paper were enunciated the convertibility and indestructibility of force, and its author referred to the mechanical equivalent of heat, merely in illustration of his principles. The essay was avowedly a kind of preliminary note. Mayer's subsequent labours conferred dignity on the theory which they illustrated. In 1845 he published an Essay on Organic Motion and Nutrition, of extraordinary merit and importance. It embraced a full development of the principles on which his calculation of 1842 was based.[2] This was followed in 1848 by an Essay on Celestial Dynamics, in which, with remarkable boldness, sagacity, and completeness, he developed the meteoric theory of solar heat. And this was followed by a fourth memoir in 1851. To these imperishable productions we shall have occasion subsequently to refer. Taking him for all in all, the right of Dr. Mayer to stand in the van of the founders of the dynamical theory of heat cannot, in my opinion, be disputed.[3]

[1] The term employed to express the lifting of 1 lb. 1 foot high.

[2] Further referred to at p. 543.

[3] Dr. Mayer ended a life of suffering on March 21, 1878.

JOULE'S EXPERIMENTAL DETERMINATION OF THE MECHANICAL EQUIVALENT OF HEAT.

On the 21st of August, 1843, Dr. Joule,[1] who had already rendered himself celebrated by his researches on the heat of the Voltaic circuit, communicated to the British Association, then meeting at Cork, a paper which was devoted, in great part, to the determination of the 'mechanical value of heat.' Joule's publication had been preceded by a long course of experiments, so that his first work and Mayer's were to a great extent contemporaneous. This elaborate investigation gave the following weights raised one foot high, as equivalent to the warming of 1 lb. of water 1° Fahrenheit.

1.	886 lbs.	5.	1,026 lbs.
2.	1,001 „	6.	587 „
3.	1,040 „	7.	742 „
4.	910 „	8.	860 „

These results, it will be observed, varied widely from each other, and partly on this account they failed to attract the attention they deserved.

From the passage of water through narrow tubes, Joule deduced an equivalent of

770 foot-pounds.

In 1844 he deduced from experiments on the condensation of air, the following equivalents to 1 lb. of water heated 1° Fahr.:

832 foot-pounds.
795 „
820 „
814 „
760 „

[1] Phil. Mag. 1843, vol. xxiii. p. 435.

As the skill of the experimenter increased, we find that the coincidence of his results became closer. In 1845 Dr. Joule deduced from experiments with water, agitated by a paddle-wheel, an equivalent of

890 foot-pounds.

Summing up his results in 1845, and taking the mean, he found the equivalent to be

817 foot-pounds.

In 1847 he found the mean of two experiments to give as equivalent,

781·8 foot-pounds.

Finally, in 1849, applying all the precautions suggested by seven years' experience, he obtained the following numbers for the mechanical equivalent of heat:

772·692,	from the friction of	water,	mean of	40	experiments.
774·083	„	„	mercury,	„	50	„
774·987	„	„	cast-iron,	„	20	„

These experiments rank among the most memorable that have ever been executed in physical science. They constitute the experimental demonstration of the dynamical theory of heat.

For reasons assigned in his paper, Joule fixes the exact equivalent at

772 foot-pounds.

Between 1870 and 1878 Dr. Joule undertook anew the determination of the mechanical equivalent of heat, and found it to be 772·55 foot-pounds.

According to the method pursued by Mayer, in 1842, the equivalent is

771·4 foot-pounds.

Such a coincidence relieves the mind of every shade of

uncertainty regarding the substantial correctness of our present mechanical equivalent of heat.

The investigations here briefly referred to, place Dr. Joule in the foremost rank of physical philosophers. Mayer *thought* his theory out, and, by an ascent of intellect which has few parallels in the history of science, rose to its grandest applications; Joule *worked* his theory out, and gave it the solidity of experimental truth. True to the speculative instinct of his country, Mayer drew large and weighty conclusions from slender premisses, creative genius atoning for the scantiness of data; while the Englishman aimed, above all things, at the firm establishment of facts. The future historian of science will not, I think, place these men in antagonism. To each belongs a reputation which can never fade, for the share he has had, not only in establishing the dynamical theory of heat, but also in leading the way towards a right appreciation of the general energies of the universe.

In lifting a pound weight then, by heat, to a height of 772 feet, an amount of heat is consumed which would raise a pound of water from 60° to 61° F.; while in falling from the same height this amount of heat would be generated. In order to imprint upon your minds the thermal effect produced by a body falling from a height, I will go through the operation of allowing a lead ball to fall from our ceiling upon this floor. That the ball is at the present moment slightly colder than the air of this room is proved by bringing the lead into contact with the thermo-pile; the deflection of the needle indicates cold. On the floor is placed a slab of iron, intended to receive the lead, and also cooler than the air of the room. At the top of the house is an assistant, who will pull up the ball by means of a string. He will not touch the ball, nor will he allow it to touch anything else. The lead now

falls, and is received upon the plate of iron. The amount of heat generated by a single shock is very small, because the height is inconsiderable; we will, therefore, allow the ball to be drawn up and to descend three or four times in succession. After the fourth collision I place the ball upon the pile; the immediate deflection of the needle direction declares the lead to be heated. According to the dynamical theory, the motion of the lead, as a mass, has been transferred to the atoms of the mass, producing among them the agitation we call heat.

We can readily calculate the amount of heat generated in this experiment. The space fallen through by the ball in each instance is 26 feet. The heat generated is proportional to the height through which the body falls. Now a ball of lead in falling through 772 feet would generate heat sufficient to raise its own temperature 30° F., its 'capacity' being $\frac{1}{30}$th of that of water: hence, in falling through 26 feet, which is in round numbers $\frac{1}{30}$th of 772, the heat generated would, if all concentrated in the lead, raise its temperature one degree. This is the amount of heat produced by a single descent of the ball, and four times this amount would, of course, be generated by four descents. The heat, however, is not all concentrated in the ball; a small portion of it belongs to the iron on which it falls.

It is needless to say, that if motion be imparted to a body by other means than gravity, the destruction of this motion also produces heat. A rifle bullet when it strikes a target is intensely heated. The mechanical equivalent of heat enables us to calculate with accuracy the amount of heat generated by the bullet, when its velocity is known. This is a point worthy of our attention, and in dealing with it permit me to address myself to those of my audience who are unacquainted with even the elements of mechanics. Everyone knows that the greater the height

from which a body falls, the greater is the force with which it strikes the earth, and that this is entirely due to the greater velocity imparted to the body in falling from the greater height. The velocity is not, however, proportional to the height. If the height be augmented four-fold, the velocity is augmented only two-fold; if the height be augmented nine-fold, the velocity is augmented only three-fold; if the height be augmented sixteen-fold, the velocity is augmented only four-fold: or, expressed generally, the height is proportional to the square of the velocity.

But the heat generated by the collision of the falling body increases simply as the height; consequently, the heat generated *increases as the square of the velocity.*

If therefore we double the velocity of a projectile, we augment the heat generated, when its motion is destroyed, four-fold; if we triple its velocity, we augment the heat nine-fold; if we quadruple the velocity, we augment the heat sixteen-fold, and so on.

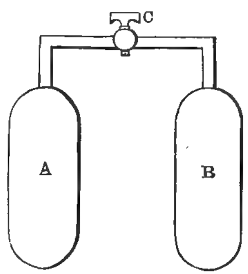
The velocity imparted to a body by gravity in falling through 772 feet is, in round numbers, 223 feet a second; that is to say, immediately before the body strikes the earth, this is its velocity. Six times this quantity, or 1,338 feet a second, would not be an inordinate velocity for a rifle bullet.

But a rifle bullet, if formed of lead, moving at a velocity of 223 feet a second, would generate on striking a target an amount of heat which, if concentrated in the bullet, would, as already shown, raise its temperature 30° F.; with 6 times this velocity it would generate 36 times the amount of heat; hence 36 times 30, or 1,080°, would represent the augmentation of the temperature of the bullet on striking a target with a velocity of 1,338 feet a second. If all the heat generated were confined to the bullet itself, this amount of heat would be sufficient to fuse

the lead. This was surmised in 1862, when I first wrote on this subject, and in the Franco-German war of 1870 the partial fusion of Chassepot bullets by their impact against bones was actually observed. Were the ball iron instead of lead, the heat generated, under the conditions supposed, would be competent to raise the temperature of the ball only about $\frac{1}{3}$rd of 1,080°, because the capacity of iron for heat is about three times that of lead.

If we could allow air to expand without any final performance of work, and if the temperature of such air remained unchanged, notwithstanding its expansion, we should have our reasonings clenched in a very conclusive manner. The experiment is not only possible, but it has been made. For its first form we are indebted to Gay-Lussac, who did not see its significance. These two copper vessels, A, B (fig. 46), are of the same size: one of them, A, is exhausted, and the other, B, filled with air. I turn the cock C; the air rushes out of B into A, until an equilibrium of pressure is established in both vessels. Experiments which we have already made inform us that the working air which remains in B must be chilled. The atoms enter A with a certain velocity, to generate which the heat of the air in B has been sacrificed; but they immediately strike against the interior surface of A, their motion of translation is arrested, and the exact quantity of heat lost by B appears in A. The contents of A and B mixed together, give air of the original temperature; there is no work performed, and there is no heat lost. This was Gay-Lussac's result. With the dynamical theory of heat in view, Dr. Joule expanded this experiment by

FIG. 46.



compressing twenty-two atmospheres of air into one of his vessels, while the other was exhausted. Placing both vessels in water, kept properly agitated, no augmentation of its temperature was observed, when the gas was allowed to stream from one vessel into the other.[1]

Let the top of the cylinder (fig. 44, p. 125) be closed, and let the half above the piston P be a perfect vacuum; let the air in the lower half be heated up to 273° C., its volume being kept constant. Supposing the piston to vanish suddenly, the air would instantly expand and fill the cylinder. The lower portion of the column would thereby be chilled, but the upper portion would be heated, and mixing both portions together, we should have the whole column at a temperature of 273°. In this case, we raise the temperature of the gas from 0° to 273°, and afterwards allow it to double its volume; the temperatures at the commencement, and at the end, are the same as when the gas expands against a constant pressure, or lifts a constant weight; but the absolute quantity of heat in the latter case is 1·421 times that employed in the former—because, in the one case, the gas performs mechanical work, while in the other it does not. It was a similar result, obtained in his experiments on steam, that caused the illustrious French physicist Regnault to give in his adhesion to the dynamical theory of heat.

Rarefaction, therefore, is not of itself sufficient to produce a lowering of the mean temperature of a mass of air. It was, and is still, a current notion, that the mere expansion of a gas produces refrigeration, no matter how that expansion may be effected. The coldness of the higher atmospheric regions has been accounted for by reference purely to the expansion of the air. It was thought that what we have called the 'capacity for heat'

[1] Phil. Mag. 1845, vol. xxvi. p. 378.

was greater in the case of the rarefied than of the unrarefied air, and that chilling must therefore be the consequence of rarefaction. Both theory and experiment prove that there is no such difference of capacity as that here assumed. The refrigeration which accompanies expansion, when air ascends from low to high elevations, is due to the consumption of heat in the work of forcing back the atmosphere surrounding and pressing against the expanding air. Where no work is performed, there is no absolute refrigeration. All this needs reflection to arrive at clearness, but every effort of this kind which you make will render your subsequent efforts easier; and should you fail, at present, to gain clearness of comprehension, I repeat my recommendation of patience. Do not quit this portion of the subject without an effort to comprehend it —wrestle with it for a time, but do not despair if you fail to arrive at clearness.

In his 'Researches in Chemistry and Physics,'[1] Faraday mentions a case in which the effect referred to a moment ago was virtually observed. His explanation is an instructive instance of the application of the material theory of heat. Referring to an observation made by him at the Portable Gas Works, in 1827, he writes: 'It frequently happens that gas, previously at the pressure of thirty atmospheres, is suddenly allowed to enter long cylinders, at which time a curious effect is observed. That end of the cylinder at which the gas enters becomes very much cooled, whilst, on the contrary, the other end acquires a considerable rise of temperature. *The effect is produced by change of capacity in the gas*; for as it enters the vessel from the parts in which it was previously confined, at a pressure of thirty atmospheres, it suddenly expands, has its capacity for heat increased, falls in temperature, and consequently

[1] P. 221.

cools that part of the vessel with which it first comes in contact. But the part which has thus taken heat from the vessel being thrust forwards to the farther extremity of the cylinder by the successive portions which enter, is there compressed by them, *has its capacity diminished*, and now gives out that heat, or a part of it, which it had the moment before absorbed.' I have italicised the phrases which express the old notion. The difference in capacity here assumed is now known to have no existence.

Our sketch of the efforts made to establish a fixed numerical relation between heat and work would not be complete without reference to the labours of M. Colding. In an essay entitled 'Theses concerning Force' presented to the Royal Society of Copenhagen in 1843, this philosopher described a series of experiments, made with the view of ascertaining the quantity of heat generated by the friction of various metals against each other, and against other substances, and of determining the amount of mechanical work consumed in its generation. In an account of his researches given by himself in the Philosophical Magazine (vol. xxvii. p. 56), he states that the result of his experiments, nearly 200 in number, was that the heat disengaged was always proportional to the mechanical energy lost. Independently of the materials by which the heat was generated, M. Colding found that an amount of heat competent to raise a pound of water 1° C. would raise a weight of one pound 1,148 feet high; a most remarkable result. M. Colding starts from the principle that 'as the forces of nature are something spiritual and immaterial—entities whereof we are cognisant only by their mastery over nature—those entities must of course be very superior to everything material in the world; and as it is obvious that it is through them only that the wisdom we perceive and admire in nature expresses itself, these powers must evidently be in relation

to the spiritual, immaterial, and intellectual power itself that guides nature in its progress; but if such is the case, it is consequently quite impossible to conceive of these forces as anything naturally mortal or perishable. Surely, therefore, the forces ought to be regarded as absolutely imperishable.' The case of M. Colding shows how a speculation, though utterly unphysical, may, by stimulating experiment, be the means of developing important physical results. Joule, it may be added, also entertained a super-physical notion which prevented him from ascribing to the Creator the generation of forces which could ever be destroyed; while contemporaneously with his physical labours Mayer was occupied with 'the transcendental truths of religion.' All these men, and Rumford may be classed along with them, drew a portion of their motive power from an ideal source.[1]

ABSOLUTE ZERO OF TEMPERATURE.

I have now to direct your attention to one other important question. We have seen the elastic force of air augmented by an increase of temperature. It has been shown that in the case of a rigid envelope we have, for every degree of temperature, a certain definite increment of elastic force, due to the augmented energy of the gaseous projectiles. Reckoning from 0° C. upwards, we find that every degree added to the temperature produces an augmentation of elastic force, equal to $\frac{1}{273}$ of that which the air possesses at 0° C., and, hence, that by raising the temperature to 273° C. we double the elastic force. An image will fix all this for ever in your minds, and enable you to see clearly certain consequences that flow from it. Supposing you have a purse containing at the outset the sum of 1*l.* 2*s.* 9*d.*, in penny pieces, and that

[1] A reference to the celebrated Sadi Carnot will be found in the preface.

you have a volume of gas at the temperature of 0° C. Let the air be gradually warmed, and for every degree of temperature imparted to it let a penny piece be added to the store already in your purse. A single degree would then raise your money to 274 pennies; ten degrees to 283 pennies; one hundred degrees to 373 pennies; while 273 degrees would augment your store to 546 pennies. You have thus, at the end, twice the sum you possessed at starting, and you have also twice the elastic force.

And now let us invert the whole proceeding. Starting with a temperature of 273° C. and with 546 pennies in the purse, let us gradually cool the air, removing a penny for every degree of temperature taken away from it. On reaching 0° C. we should obviously have 273 pennies in our purse. But there is no magic in the temperature 0° C. which could cause the value of a degree of chilling to change at that particular point. Below it, as above, the value will be still a penny. Let us, then, continue the cooling process, throwing away a penny per degree as before. One degree of chilling would lessen our cash one penny, 10° ten pennies, 100° one hundred pennies, while 273° of chilling would entirely empty our purse. The diminution of pressure would here follow the same rule as the diminution of cash, were it not that the molecular forces which are insensible at higher temperatures come into play. At 273° below zero centigrade we should empty at the same time our purse of pennies and our air of pressure. The air would then have sunk to *the absolute zero of temperature.* The absolute zero has never been attained, and long before reaching it all gases would become liquids, ceasing to follow the law of diminution of elastic force which we have here applied. Still the determination of the absolute zero is a point of great importance, constituting as it does a real standard to which temperatures can be referred.

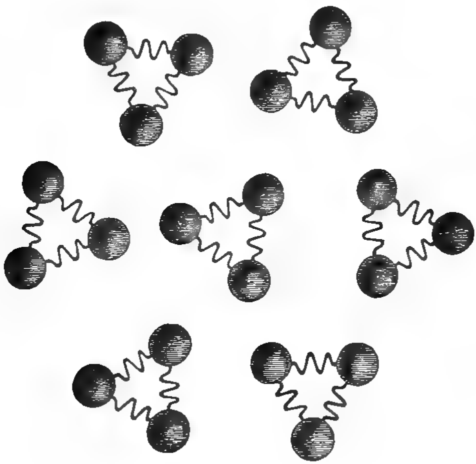
Starting, for example, from the true zero, the conventional centigrade zero would be 273°; and reckoning from the true zero, we have the fundamental but extremely simple law that the pressure of a perfect gas is proportional to the absolute temperature. But the pressure depends wholly upon the projectile force of the molecules, which is commonly called *vis viva*; hence the absolute temperature is proportional to the *vis viva* arising from the translatory or projectile motion.

But there may, and indeed *must*, be other motions than that of translation among the molecules. Consider the case of two billiard balls approaching each other and coming into collision. If their shock be direct or central, they will recoil exactly as they approached; but if the collision, instead of being central, be oblique, a motion of rotation is added to the motion of translation. It is thus with atoms when they fly singly through space. The motion of translation, therefore, does not express the entire motion; nor does the *temperature*, which is due to the motion of translation, express the entire *heat*. This is the case to a much greater extent in compound gases than in simple ones. The compound molecules fly through space; but a moment's reflection will satisfy you that this translatory flight and mutual collision must immediately set up other motions. Take, for example, an assemblage of molecules each composed of three atoms held within a certain distance of each other by a force of attraction, and prevented from coming into actual contact by a force of repulsion. These two forces act the part of elastic springs, and are represented by such springs in the annexed figure (fig. 47). Such molecules flying through space and clashing against each other must add to their translatory motion, as wholes, a vibratory motion of their constituent atoms. Now this vibration has nothing to do with the pressure or expansion of the gas, which is taken as a

measure of its temperature. The vibration, in fact, though a portion of the heat, is not the portion by which temperature is determined. Heat and temperature, therefore, are two distinct things.

It is, however, manifest that the swifter the flying motion, the more energetic will be the collisions, and consequently the more intense the vibratory motion of the atoms. Indeed, it stands to reason that the two kinds of motion will be proportional to each other. Other con-

FIG. 47.



siderations render it certain that they are so, so that in comparative experiments we may take the *temperatures* as representing the relative *total heats*.

LIQUEFACTION OF GASES, INCLUDING OXYGEN, HYDROGEN, AND ATMOSPHERIC AIR.

I have already referred to certain gases as 'imperfect,' and as having been liquefied; and a moment ago I referred to the probability of all gases assuming the liquid

form before reaching the absolute zero of temperature. Towards the end of 1877 universal interest was excited by the intelligence that oxygen, hydrogen, and air had been liquefied. It had been long foreseen that such liquefaction would sooner or later be effected. Lavoisier, for example, speculated on what would occur if the earth were transported to the distance of Saturn from the sun, and he concluded that under such circumstances our refrigerated atmosphere would change its gaseous condition and precipitate itself as a liquid or solid upon the surface of the earth. The subject soon passed beyond the domain of speculation; and it is probable in a high degree that prior to the year 1800 Monge and Clouet, combining refrigeration with compression, succeeded in liquefying sulphurous acid gas. This, I say, is in a high degree probable; but it is certain that, in 1805, Northmore succeeded in liquefying chlorine. Faraday pointed this out in 1824; and in 1836, in answer to criticism, he expressed himself thus: 'When I discovered in the course of the same year that *neither I nor Sir Humphry Davy* had the merit of first condensing the gases, and especially chlorine, I hastened to perform what I thought right, and had great pleasure in spontaneously doing justice and honour to those who deserved it.'

Faraday's own condensation of chlorine was far more than a revival of the experiment of Northmore. The history of this experiment is well known. Scheele had discovered chlorine in 1774. Davy had proved it to be an element in 1810, and had also shown that the solid which passed in those days among chemists as pure chlorine, was a hydrate of chlorine. Crystals of this hydrate were collected by Faraday in the spring of 1823. He analysed the substance, and submitted the analysis to Davy, who suggested to him to heat the hydrate in a sealed glass tube. Chlorine gas is liberated by such

heating, and may be powerfully self-compressed. On the 5th of March Dr. Paris had been invited to dine with Davy, and on his way to dinner he turned into the laboratory of the Royal Institution, where he found Faraday with a sealed tube in his hand containing an oily liquid. This was the tube containing the hydrate which Faraday had just heated. Paris rallied the young chemist for using dirty tubes in his experiments. On filing off the sealed end of the tube, the liquid escaped with explosive violence. On the following day Dr. Paris received a note from Faraday, stating: 'The oil you noticed yesterday turns out to be liquid chlorine.' The gas had been liquefied by self-compression. At the same time Davy liquefied muriatic gas by the same method, and Faraday soon afterwards, invoking the aid of ordinary mechanical pressure, added five other gases to the list of those liquefied. In 1844 he returned to the subject, and extended its boundaries. He tried oxygen and failed, but he held fast to the belief that the day would come when it, and the other so-called permanent gases, would be converted into liquids.

Colladon and others tried such experiments, but failed. Mr. Perkins subjected air to a pressure of 1,100 atmospheres, and thought that he had liquefied it. This, however, was not the case. Natterer of Vienna subsequently took up the question, bringing enormous forces to bear. He subjected oxygen to a pressure of 1,354 atmospheres, while in the case of hydrogen and nitrogen he applied a pressure of 2,790 atmospheres. In 1856, in company with Dr. Frankland, I paid a visit to M. Natterer in Vienna, and he then showed us a small steel cylinder which had been actually shortened by the force with which it was pressed against a resisting gas.

The subject slumbered for a time, but attention was again forcibly called to the general question by the researches

of Dr. Andrews, of Belfast, which threw a flood of light on the relations of the liquid to the gaseous states of matter.

In 1877 M. Cailletet had liquefied nitric oxide and acetylene, and on the 2nd of December he placed in the hands of M. Henri Saint-Claire Deville a note wherein, in cautious but distinct terms, he announced the liquefaction of oxygen. On the 16th of the same month he repeated his experiments in the presence of several members of the Institute. His plan of operation involved the application of the principle of refrigeration by expansion which we have already illustrated. By instruments of great strength, and supreme accuracy of fit, he was able to subject a volume of oxygen gas to a pressure of 300 atmospheres. He might have multiplied this pressure tenfold without liquefying the gas, but instead of augmenting the pressure, he suddenly released the gas from the pressure imposed upon it. It forcibly expanded, and the cold of expansion caused the gas to precipitate itself as a cloud, which the eminent men who witnessed the experiment agreed in pronouncing liquid oxygen. He subsequently applied the same method with success to nitrogen, hydrogen, and air; all of which, through the combination of pressure with sudden release from pressure, were caused to precipitate themselves in clouds.

M. Raoul Pictet followed another method. The pressure at which carbonic acid gas liquefies varies with the temperature of the gas; and M. Pictet's first object was to produce a vessel cooled to such a degree that large quantities of carbonic acid gas could be liquefied in it by a moderate pressure. To chill his vessel he employed liquid sulphurous acid, which, by means of a pump, was caused to travel in a circuit in which it was alternately vaporised and recondensed. Through the vaporisation a cold of 65° below 0° C. was produced; and at this temperature the carbonic acid was liquefied by a pressure of

from four to six atmospheres. By the evaporation of the liquid carbonic acid a cold of $-130°$ was produced.[1] A strong tube, surrounded by an envelope of this temperature, was connected with an iron vessel containing the materials necessary for the generation of oxygen. The gas being liberated entered the chilled tube, where it became more and more subjected to its own pressure. The method, it will be observed, is substantially that which Faraday and Davy had pursued with chlorine and muriatic gas, the aim of M. Pictet being to liquefy the oxygen by self-compression. At a pressure of 470 atmospheres with a temperature of $-130°$, the deportment of the oxygen showed that it must have been liquefied. Hydrogen was subsequently liquefied by the same process. It is impossible not to admire the penetration, the ardour, and indeed the courage displayed by M. Raoul Pictet in conducting this memorable experiment.[2]

[1] The cause of this lowering of temperature will be set forth in a future lecture.

[2] A telegram announcing his result was received by me from M. Raoul Pictet on Christmas morning, 1877.

LECTURE VI.

INFLUENCE OF PRESSURE ON LIQUEFACTION AND SOLIDIFICATION—LIQUEFACTION OF ICE BY PRESSURE—DISSECTION OF ICE BY A CALORIFIC BEAM—LIQUID FLOWERS AND THEIR CENTRAL SPOT—MECHANICAL PROPERTIES OF WATER PURGED OF AIR—VAPORISATION OF WATER, THE BOILING POINT—CONVERSION OF HEAT INTO WORK IN THE STEAM-ENGINE: THE GEYSERS OF ICELAND.

IN the vast majority of cases the passage of bodies from the liquid to the solid state is accompanied by contraction. Over hot water contained in a round glass dish, I pour a quantity of melted wax. The wax forms a liquid layer nearly half an inch thick above the water. We will suffer both water and wax to cool; in cooling, the wax, which now overspreads the entire surface and is attached all round to the glass, will retreat, and we shall finally obtain a cake of considerably smaller area than the dish.

Reversing the process, wax, in passing from the solid to the liquid state, *expands*, a certain play between the molecules being here necessary to the condition of liquidity. Suppose we resist the expansion of the wax, what would the effect be upon the temperature of liquefaction? When the wax is free, the heat has only to conquer the attraction of the molecules; but when expansion is resisted the difficulty of liquefaction is increased. By a mere process of reasoning, we should be led to infer that a greater amount of heat would be required to melt the wax under pressure than that which suffices when molecular attraction only is overcome; in other words, that the point of fusion of the

wax will be *elevated* by pressure. This reasoning is completely justified by experiment. Messrs. Hopkins and Fairbairn raised, by pressure, the melting point of some substances, which, like wax, contract considerably on solidifying, as much as 20° and 30° Fahr.

The experiments here referred to connect themselves with a very remarkable speculation. The earth is known gradually to augment in temperature as we pierce it deeper, and the depth has been calculated at which all known terrestrial bodies would be in a state of fusion. Owing, however, to the enormous pressure of the superincumbent layers, the deeper strata, according to Mr. Hopkins, would require a far higher temperature to fuse them than would suffice to fuse the strata near the earth's surface. Hence he inferred that the solid crust must have a considerably greater thickness than that given by a calculation which assumes the fusing points of the superficial and he deeper strata to be the same. Mr. Hopkins therefore rejected the estimate of geologists that the earth could be a molten nucleus covered by a crust only 100 miles in thickness, concluding that the depth of the crust must be at least 800 miles. Sir William Thomson considers it 'extremely improbable that any crust thinner than 2,000 or 2,500 miles could maintain its figure with sufficient rigidity against the tide-generating forces of sun and moon, to allow the phenomena of the ocean tides and of precession and nutation to be as they now are.'

The deportment of ice is opposed to that of wax. Ice on liquefying *contracts*; in the arrangement of its molecules to form a solid, more room is required than they need in the neighbouring liquid state. No doubt this is due to crystalline arrangement. When the crystallising force comes into play, the attracting poles of the molecules unite so as to leave larger interatomic spaces in the mass. We may, as already explained, suppose the molecules to

attach themselves by their corners; and, in turning corner to corner, to cause a virtual augmentation of bulk. At all events, the molecules retreat from each other when solidification sets in. It is evident that pressure, in this case, would resist the expansion which is necessary to solidification, and hence the tendency of pressure, in the case of water, is to keep it liquid. Thus reasoning, we should be led to the conclusion that the fusing points of substances which expand on solidifying are *lowered* by pressure.

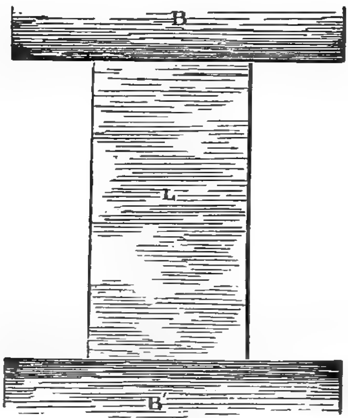
Professor James Thomson was the first to infer, from a principle enunciated by Carnot, this lowering of the freezing point, and his theoretic deduction was completely verified by the experiments of Sir William Thomson. Soon afterwards, Professor Clausius proved the result to be in harmony with the mechanical theory of heat.

Before you is a small pillar of clear ice an inch and a half in height, and about a square inch in cross section. Its present temperature is 0° C. If that ice be subjected to pressure it will melt at a temperature under 0° C.; hence the temperature which it now possesses is in excess of that at which it will melt under pressure. The ice is cut so that its planes of freezing are perpendicular to the height of the pillar. I set the column of ice, L (fig. 48), upright between two slabs of boxwood, B′ B, and place the whole between the plates of a small hydraulic press. A strong luminous beam passes through the ice; the beam having been previously sent through water to deprive it of the power of melting the ice. The sifted light[1] now passes through the substance without causing fusion. In front of the press is placed a lens, and by it a magnified image of the ice is projected upon the screen. Working the arm of the press, I gently squeeze the pillar

[1] The 'sifting' of a calorific beam will be fully explained and illustrated in a subsequent Lecture.

of ice between the two slabs of boxwood. Dark streaks soon begin to draw themselves across the substance, at right angles to the direction of pressure. Right in the middle of the mass they are appearing; and as the pressure continues, the old streaks expand and new ones are developed. The entire column of ice is now scarred by these transverse striæ. What are they? They are simply *liquid layers* foreshortened, and when you examine this column under pressure, you see the solid falling with com-

FIG. 48.

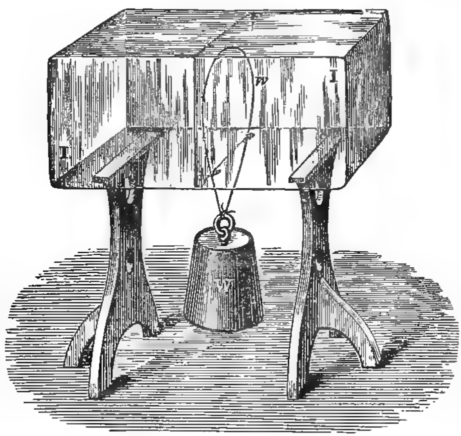


motion into the liquid condition. We have here liquefied the ice in planes perpendicular to the pressure, and these liquid planes interspersed throughout the mass give it this laminated appearance.

If instead of diffusing the pressure over surfaces of considerable extent, we concentrate it on a small surface, the liquefaction will of course be more rapid, and this is what Mr. Bottomley has done in an experiment of singular beauty and interest. Let us place on two wooden supports the two ends of a bar of ice, I I (fig. 49), 10 or 12 inches long, 4 or 5 inches deep, and 3 or 4 wide, and let us loop

over its middle a copper wire one-twentieth, or even one-tenth, of an inch in thickness. Connecting the two ends of the wire together, and suspending from it a weight w, of 12 or 14 pounds, the whole pressure of this weight is concentrated on the ice which supports the wire. What is the consequence? The ice underneath the wire liquefies; the water of liquefaction escapes round the wire, but the moment it is relieved from the pressure it re-freezes, and

Fig. 49.



round about the wire, even before it has quite entered the ice, you have a frozen casing. The wire continues to sink; the water incessantly escapes, freezing, as it does so, behind the wire. In half an hour the weight falls; for the wire has gone clean through the ice. We can plainly see where it has passed; but the two severed pieces of ice are so firmly frozen together that they will break elsewhere quite as readily as along the surface of regelation.

Another beautiful experiment bearing upon this point has been made by M. Boussingault. He accurately filled

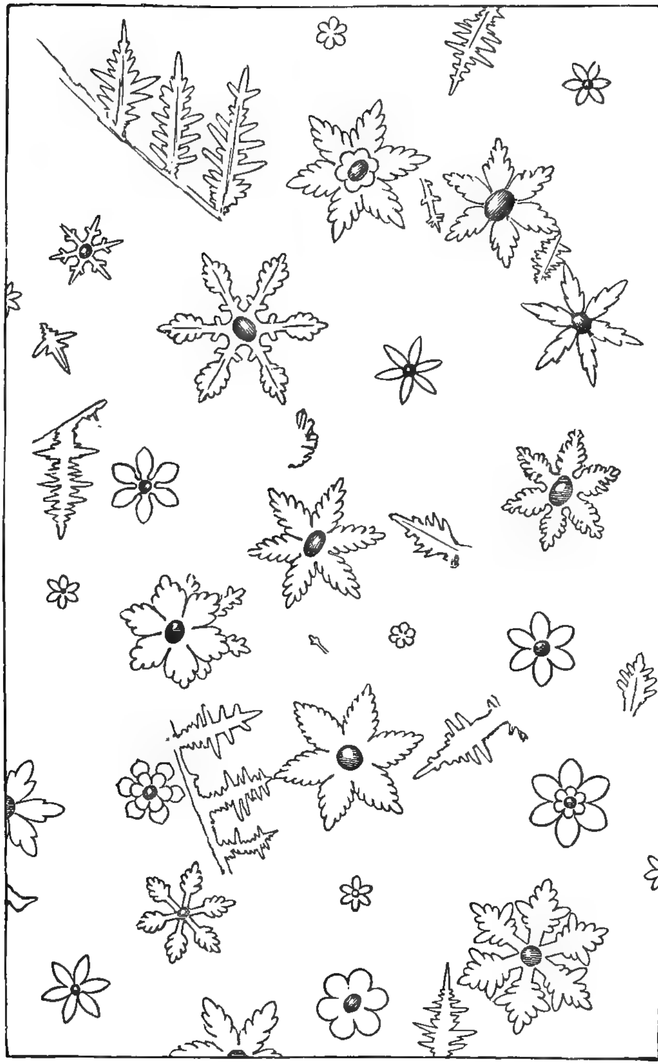
a hollow steel cylinder with ice-cold water and chilled it still further. In this case the strong steel resisted the expansion necessary to the solidification of the water, which, in consequence, remained liquid at a temperature of more than 30° Fahr. below the ordinary freezing point. When the cylinder was shaken, a bullet within it rattled, showing that the water was still liquid at this temperature. On opening a tap the liquid, relieved of the pressure, was instantly converted into ice. M. Mousson, it may be added, has liquefied large quantities of ice by pressure.

DISSECTION OF ICE BY RADIANT HEAT.

Whether as a solid, a liquid, or a gas, water is one of the most wonderful substances in nature. Let us consider it a little further. At all temperatures above 32° F. or 0° C. the motion of heat is sufficient to prevent its molecules from rigidly uniting. But at 0° C., the heat-motion is so reduced that the molecules begin to aggregate to a solid. This, however, is a union according to law. To many persons here present a block of ice may seem of no more interest and beauty than a block of glass; but in reality it bears the same relation to glass that orchestral harmony does to the cries of a market-place. The ice is music, the glass is noise; the ice is order, the glass is confusion. In the glass, molecular forces constitute an inextricably entangled skein; in the ice they are woven to a symmetric texture, the beauty of which I will now try to make evident to you.

In the solar beam—or, failing that, in the beam of our electric lamp—we have an analyst competent to perform the work here contemplated. Removing the agent by which the beam was sifted in the last experiment, I send the rays direct from the lamp through a slab of pellucid

FIG. 50.



ice. It will take the crystal edifice to pieces by accurately reversing the order of its architecture. Silently and symmetrically the crystallising force built the molecules up, silently and symmetrically the electric beam will take them down. Compare the radiant beam before it enters the ice with the beam after its passage through the substance. To the eye there is no difference: the light is not sensibly diminished. Not so with the heat. As a thermic agent, the beam, before entering, is far more powerful than after its emergence. A portion of it has been arrested in the ice, and that portion is to be our working analyst. I place a lens in front of the ice, and cast a magnified image of the slab upon the screen. Observe that image (fig. 50). Here we have a star, and there a star; and as the action continues, the ice appears to resolve itself into stars, each one possessing six rays, each one resembling a beautiful flower of six petals. When the lens is shifted to and fro, new stars are brought into view; and as the action continues, the edges of the petals become serrated, and spread themselves out like fern-leaves upon the screen. Probably few here present were aware of the beauty latent in a block of common ice. And only think of lavish Nature operating thus throughout the world. Every molecule of the solid ice which sheets the frozen lakes of the North has been fixed according to this law. Nature, the poet says, 'lays her beams in music,' and it is the function of science to purify our organs, so as to enable us to hear the strain. Beautiful as these are, I have frequently obtained far more perfect figures than those here presented to you.

There are two points connected with this experiment, of great minuteness, but of great interest. You see these flowers by transmitted light—by the light, that is, which has passed through both the flowers and the ice; and you see a bubble in the centre of each flower. In many cases

the bubble moves before your eyes. When you examine the flowers by allowing a beam to be reflected from them to your eye, you find in the centre of each flower a spot shining with the lustre of burnished silver. You might be disposed to think this spot a bubble of air; but you can, by immersing it in hot water, melt away the circumjacent ice. The moment the spot is thus laid bare, it collapses, and no trace of a bubble is to be seen. *The spot is a vacuum.* We know that ice in melting contracts; hence the water of these flowers cannot quite fill the space of the ice by the fusion of which they are produced; a vacuum necessarily accompanies the formation of every liquid flower.

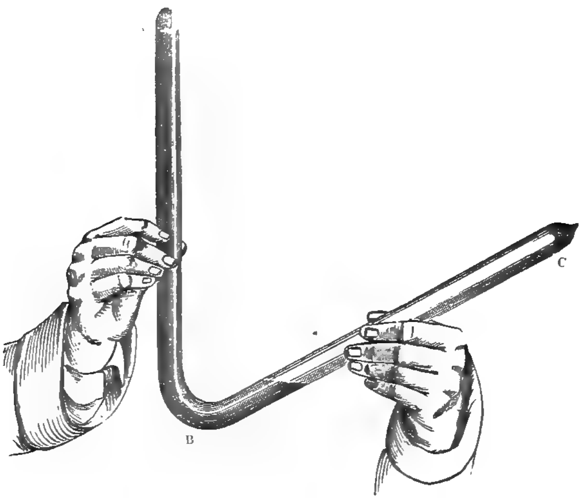
SOUNDS HEARD IN THE DISSECTION OF ICE; THEIR RELATION TO DONNY'S EXPERIMENTS.

When I first observed these beautiful figures, at the moment when the central spot appeared like a point of light suddenly formed within the ice, a click was heard. At first I thought it might be imagination which associated sound with the appearance of the spot, as people who see meteors sometimes imagine a rushing noise when they really hear none. The click, however, was a reality; and if you allow me, I will now conduct you from this trivial fact through a series of interesting phenomena to a far-distant question of practical science.

Gases are soluble in water—some more, some less. Oxygen and nitrogen are thus soluble, and the same is true of the atmospheric air formed by their mixture. This dissolved air is a powerful enemy to the cohesion of water; and when it is removed, the embrace of the liquid molecules is greatly strengthened. Nature had in this way liquefied these permanent gases ages before they were liquefied by man, thus providing us with a measure

of the power of molecular force, as compared with mechanical force. By boiling you may liberate this imprisoned air. On heating a flask of water, air-bubbles are seen crowding on its sides, long before it boils, rising through the liquid without condensation, and often floating on the top. The presence of this air in the water promotes the ebullition of the liquid. It acts as a kind of elastic spring, pushing the molecules apart, and thus helping them to take the gaseous form.

FIG. 51.



When this antagonist to their intimate union is removed, the molecules lock themselves together in a far tighter embrace. One effect of the withdrawal of the elastic buffer is, that the water falls with the sound of a solid body, producing what is called 'the water-hammer.' You hear how the purged liquid rings against the end of a tube containing it, when the tube is turned upside down. This other tube, A B C (fig. 51), bent into the form of a V, is intended to show how the cohesion of the water is affected by

long-continued boiling. The water which partially fills the bent tube is first brought into one arm of the V. On tapping the end of this arm against the table you hear, at first, a loose and jingling sound. As long as that jingle is heard the water is not in true contact with the interior surface of the tube. As the tapping continues the sound changes, becoming hard like that of solid against solid. I now raise the tube, and turn the column of water upside down; it stands unsupported in the arm A B. Freed from air its particles cling so tenaciously to the tube and lock themselves so firmly together, that it refuses to fall.

This cohesion, moreover, enables the liquid to resist ebullition. Water freed of its air can be raised to a temperature of 60° or 80° Fahr., above its ordinary boiling point, without boiling. The locked atoms finally part company, but they do so with the violence of a spring which suddenly breaks under strong tension, and ebullition is converted into explosion. To M. Donny, of Ghent, we are indebted for the discovery of this interesting property of water.

Turn we now to our ice. Water in freezing completely excludes the air from its structure. All foreign bodies are indeed rejected, air among the rest. If, then, we melt a piece of pure ice where air cannot approach it, we shall obtain water in its most highly cohesive condition; and such water ought, if heated, to show the effects mentioned. That it does so has been proved by Faraday. He melted ice under spirit of turpentine, and found that the liquid thus formed could be heated far beyond its boiling point, and that the rupture of the liquid, by heating, took place with almost explosive violence. Let us apply these facts to the six-petaled ice-flowers, and their little central spot. They are formed in a place where no air can come. Imagine the flower forming, and

gradually augmenting in size. The cohesion of the liquid is so great, that it will pull the walls of its ice-chamber together, or even expand its own volume, sooner than give way. But, as its size augments, the space which it tries to occupy becomes too large for it, until finally the liquid snaps with an audible click, and a vacuum is formed.

Let us take our final glance at this web of relations. It is very remarkable that a great number of locomotives have exploded on quitting the shed where they had remained for a time quiescent, and just as the engineer turned on the steam. Now, if a locomotive has been boiling sufficiently long to expel the air contained in its water, the liquid will possess, in a greater or less degree, the high cohesive quality to which your attention has been drawn. It is at least conceivable, that while resting, previous to starting, an excess of heat might be thus stored up in the boiler, and, if stored up, the mechanical act of turning on the steam would produce the rupture of the cohesion, and steam of explosive force would instantly be generated. I do not say this *is* the case; but who can say it is *not* the case? We have been dealing throughout with a real agency, which is certainly competent, if its power be invoked, to produce the effects ascribed to it.

VAPORISATION OF WATER—THE BOILING POINT.

As you add heat, or, in other words, molecular motion, to water, the molecules from its free surface fly off in augmented numbers. You at length approach what is called the *boiling point* of the liquid, where the conversion into vapour is not confined to the free surface, but is most copious at the bottom of the vessel where the heat is applied. When water boils in a glass beaker, the steam is seen rising in bubbles from the bottom to the top, where it often floats for

a time, inclosed above by a dome-shaped film. To produce these bubbles certain resistances must be overcome. First, we have the adhesion of the water to the vessel which contains it, and this force varies with the substance of the vessel. In the case of a glass vessel, for example, the boiling point may be raised two or three degrees by adhesion; while in metal vessels this is impossible. The adhesion is often overcome by fits and starts, which may be so augmented by the introduction of certain salts into the liquid, that a loud bumping sound accompanies the ebullition; the detachment is in some cases so sudden and violent, as to cause the liquid to leap bodily out of the vessel.

A second antagonism to the boiling of the liquid is the attraction of the molecules for each other; a force which, as we have seen, may become very powerful when the liquid is purged of air. This is not only true of water, but of other liquids—of all ethers and alcohols, for example. If we connect a small flask containing ether or alcohol with an air-pump, a violent ebullition occurs in the liquid when the pump is first worked; but after the air has been removed from the liquid, we may, in many cases, continue to work the pump without producing any sensible ebullition; the free surface alone of the liquid yielding vapour.

But in order that steam should exist in bubbles, in the interior of a mass of liquid, it must be able to resist two other things—the weight of the water above it, and the weight of the atmosphere above the water. What the atmosphere is competent to do may be thus illustrated. This tin cylinder contains a little water, which is kept boiling by a small lamp. At the present moment all the space above the water is filled with steam, which issues from a stopcock. I shut the cock, withdraw the lamp, and pour cold water upon the tin vessel. The steam within it is

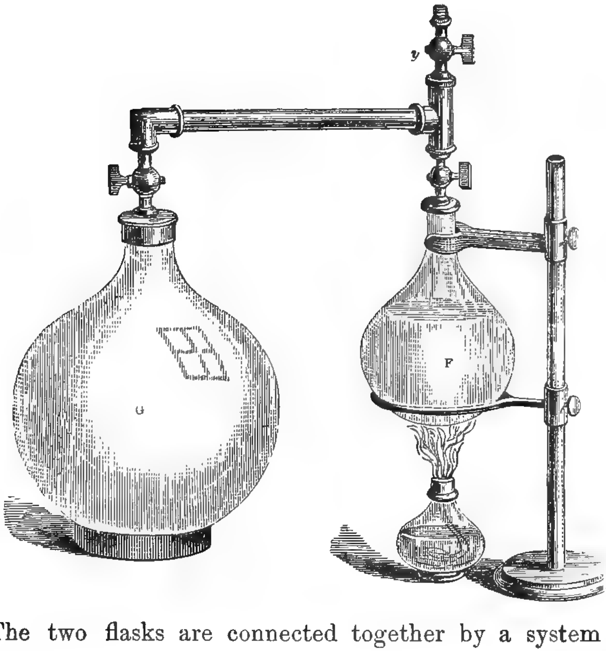
condensed, the elastic cushion which pushed the sides outwards in opposition to the pressure of the atmosphere is withdrawn, and the sides of the vessel are crushed and crumpled up by the atmospheric pressure. This pressure amounts to 15 lbs. on every square inch: how, then, can a thing so frail as a bubble of steam exist on the surface of boiling water? Simply because the elastic force of the steam within the bubble is exactly equal to that of the atmosphere without; the liquid film is pressed between two elastic cushions which exactly neutralise each other. If the steam were predominant, the bubble would burst from within outwards; if the air were predominant, the bubble would be crushed inwards. Here, then, we have the true definition of the boiling point of a liquid. It is that temperature at which the tension of its vapour exactly balances the pressure of the atmosphere.

As we ascend a mountain, the pressure of the atmosphere above us diminishes, and the boiling point is correspondingly lowered. On an August morning in 1859 I found the temperature of boiling water on the summit of Mont Blanc to be 184·95° F.; that is, about twenty-seven degrees lower than the boiling point at the sea level. On August 3, 1858, the temperature of boiling water on the summit of the Finsteraarhorn was 187° F. On August 10, 1858, the boiling point on the summit of Monte Rosa was 184·92° F. The boiling point on Monte Rosa is shown by these observations to be almost the same as it was found to be on Mont Blanc, though the latter exceeds the former in height by 500 feet. The fluctuations of the barometer are, however, quite sufficient to account for this anomaly. The lowering of the boiling point is about 1° F. for every 590 feet of elevation; and from the temperature at which water boils, we may approximately infer the height. It is sometimes said, that to make good tea at low levels, boiling water is essential; if this were so,

it is evident that the beverage could not be procured, in all its excellence, at the higher stations of the Alps. The tea there, however, is, or at all events may be, excellent.

Our next experiment will illustrate the dependence of the boiling point on external pressure. The flask F (fig. 52) contains water; while from a second and much larger one, G, the air has been removed by an air-pump

FIG. 52.



The two flasks are connected together by a system of cocks, which enables me to establish a communication between them. The water in the small flask has been boiling for some time, the steam generated escaping through the cock *y*. I now remove the spirit-lamp, and turn the cock *y*, so as to shut out the air. The water ceases to boil, and pure steam now fills the flask above it. We will give the water time to cool a little. At

intervals you see a bubble of steam rising, because the pressure of the vapour on the water in the flask F is gradually becoming less through its own slow condensation When this is hastened by pouring cold water on the flask, the bubbles are more copiously generated. By plunging the flask bodily into cold water we might cause it to boil violently. The water in F is now at rest, and some degrees below its ordinary boiling point. I turn the cock *c*, which opens a way for the escape of the vapour into the exhausted vessel G; the moment the pressure is diminished ebullition begins in F; while the condensed steam showers in a kind of rain against the sides of the vessel G. By keeping G cool, and thereby preventing the vapour in it from reacting upon the surface of the water in F, we can cause the smaller flask to bubble and boil for a considerable time.

Through high heating, the elastic force of steam may be rendered enormous. The Marquis of Worcester burst cannon with it, and our calamitous boiler explosions are so many illustrations of its power. By the skill of man this mighty agent has been controlled; with it Denis Papin raised a piston, which, when the steam was condensed, was pressed down again by the atmosphere; Savery and Newcomen first turned steam to practical account, and James Watt completed this grand application of the moving power of heat. Pushing the piston up by steam, while the space above the piston is in communication with a condenser or with the free air; and again pushing down the piston, while the space below it is in communication with a condenser or with the air, we obtain a simple to-and-fro motion, which, by mechanical arrangements, may be made to take any form we please.

But here, as elsewhere, the principle of conservation is illustrated. For every stroke of work done by the steam-engine, for every weight that it lifts, and for every wheel that it sets in motion, an equivalent quantity of heat dis-

appears. A ton of coal furnishes by its combustion a certain definite amount of heat. Let this quantity of coal be applied to working a steam-engine; and let all the heat communicated to both engine and condenser, and all the heat lost by radiation and by contact with the air, be collected; it will fall short of the quantity produced by the simple combustion of the ton of coal, by an amount exactly equivalent to the work performed. Suppose that work to consist in lifting a weight of 7,720 lbs. a foot high; the heat produced by the coal would fall short of its true amount by a quantity just sufficient to warm a pound of water 10° F. In an elaborate series of experiments, executed with extraordinary assiduity, and on a grand scale, by M. Hirn of Colmar, this theoretic deduction has been reduced to fact.

In the steam-engine employed by M. Hirn the steam left the boiler and entered the cylinder at a temperature of 146° C. The temperature of his condenser was 34° C. The steam was worked expansively; that is to say, it was permitted to enter the cylinder and exert its full pressure until the piston was raised through a certain fraction of its range. The steam was then cut off, the piston being urged through the remainder of its course by the expansive force of the steam already in the cylinder.

In this case the space above the piston was connected with the condenser; and if the expansion were perfect the vapour underneath the piston at the moment it reached the highest point of its course would have the pressure corresponding to the temperature of the condenser. Were the expansion perfect, and did the expanded vapour all remain in the state of vapour, the experiments of M. Regnault would enable us to calculate the fraction of the total heat converted into work. By such a calculation, and not without a feeling of astonishment at its smallness, it was found that in the experiments of M. Hirn, the heat con-

verted into work ought not to amount to $\frac{1}{19}$th of the whole.

But as a matter of fact M. Hirn found that $\frac{1}{8}$th of the heat borrowed from the steam in the boiler was converted into mechanical effect. Thus experiment was at variance with calculation. A theoretic conclusion arrived at independently, and almost simultaneously, by Rankine and by Clausius, two of the founders of the mechanical theory of heat, reveals the cause of this discrepancy. In calculating the heat possessed by the vapour as it enters the condenser, it was assumed that the whole of the vapour coming from the boiler remained during its expansion in the vaporous condition. This Rankine and Clausius proved that it could not do. They showed that when saturated steam expands, as in M. Hirn's experiments, it is in part precipitated, thus yielding up a portion of the heat of vaporisation, which portion is available for work. Indeed, before anything correct was known about its cause, mechanical engineers met the nuisance arising from the water of condensation by surrounding the cylinder with a jacket of hot steam from the boiler. The mixture of vapour and liquid entering the condenser, after the expansion, possesses less heat than if it were all vapour; a greater amount of heat than that given by calculation being converted into work. I may add, that the precipitation of the steam during its expansion was demonstrated experimentally by M. Hirn.

But even the conversion of $12\frac{1}{2}$ per cent. of the total heat into work implies enormous loss. Nor is this loss to be avoided in the steam-engine. For the amount of heat converted into work depends upon two things: the temperature of the steam as it enters the cylinder, and its temperature as it enters the condenser. The farther the initial and the final temperatures are apart, the greater is the amount of heat converted into work; but to convert

all the heat into work, a condenser kept at the absolute zero of temperature would be required. As already explained, the absolute temperature of a body is its temperature reckoned from the absolute zero. Let T represent the initial temperature of the steam, and t its final temperature, both reckoned from the absolute zero; then the proportion of the total heat converted into work cannot, under the most favourable conditions, exceed the fraction

$$\frac{T-t}{T}.$$

My object, however, at present is to deal with nature rather than art, and I am compelled to pass quickly over the triumphs of man's skill in the application of steam to the purposes of life. Those who have walked through the workshops of Woolwich, or through any of our great factories where machinery is extensively employed, will have been sufficiently impressed with the aid which the mighty power of heat renders to man. Let it be remembered, that every wheel which revolves, every chisel, and plane, and punch, which passes through solid iron as if it were so much cheese, derives its moving energy from the clashing atoms in the furnace. The motion of these atoms is communicated to the boiler, thence to the water, whose molecules are shaken asunder, flying from each other with a repellent energy commensurate with the heat communicated. The steam is simply the apparatus, through the intermediation of which, the atomic motion is converted into mechanical motion. And the motion thus generated always, in the long run, reproduces its parent. Look at the planing tools and boring instruments—streams of water gush over them to keep them cool. Take up the curled iron shavings which the planing tool has pared off; you cannot hold them in your hand, they are so hot. Here the moving force is restored to its first form; the energy of the machine has been consumed

in reproducing the power from which that energy was derived.

THE GEYSERS OF ICELAND.

Let me now direct your attention to a natural steam-engine, which long held a place among the wonders of the world—the Great Geyser of Iceland. The surface of that country gradually rises from the coast towards the centre, where the general level is about 2,000 feet above the sea. On this, as on a pedestal, are planted the Jökull, or icy mountains of the island, which extend both ways in a north-easterly direction. Along this chain occur the active volcanoes of Iceland, and the thermal springs follow the same general direction. From the ridges and chasms which diverge from the mountains, enormous masses of steam issue at intervals, and when the escape occurs at the mouth of a cavern, the resonance of the cave often raises the sound of the steam to the loudness of thunder. Lower down, in the more porous strata, are to be found smoking mud pools, where a blue-black aluminous paste is boiled, rising at times in huge bubbles, which, on bursting, scatter their slimy spray around. From the base of the hills upwards extend the glaciers, and above these are the snow-fields which crown the summits. From the arches and fissures of the glaciers, vast masses of water issue, falling at times in cascades over walls of ice, and spreading for miles over the country before they find definite outlet. Extensive morasses are thus formed. Intercepted by the cracks and fissures of the land, a portion of the water finds its way to the heated rocks beneath; and here, meeting with the volcanic gases which traverse these underground regions, both travel on together, to issue, at the first convenient opportunity, either as an eruption of steam or as a boiling spring.

The most famous of these springs is the Great Geyser.

It consists of a tube, seventy-four feet deep and ten feet wide. The tube is surmounted by a basin, which measures from north to south fifty-two feet across and from east to west sixty feet. The interior of the tube and basin is coated with a beautiful smooth siliceous plaster, so hard as to resist the blows of a hammer; and the first question is, How was this wonderful tube constructed—how was this perfect plaster laid on? Chemical analysis shows that the water holds silica in solution, and it might therefore be conjectured that the water had deposited silica against the sides of the tube and basin. But such is not the case. The water deposits no sediment; no matter how long it may be kept, no solid substance is separated from it. I have here a specimen which has been bottled up and preserved for years, as clear as crystal, without showing the slightest tendency to form a precipitate. To answer the question in this way would moreover assume that the shaft was formed by some foreign agency, the mineral water merely lining it. The geyser-basin, however, rests upon the summit of a mound about forty feet high, and it is evident, from mere inspection, that the mound has been deposited by the geyser. But in building up this mound the spring must have formed the tube which perforates the mound; hence the suggestion that the geyser is the architect of its own tube.

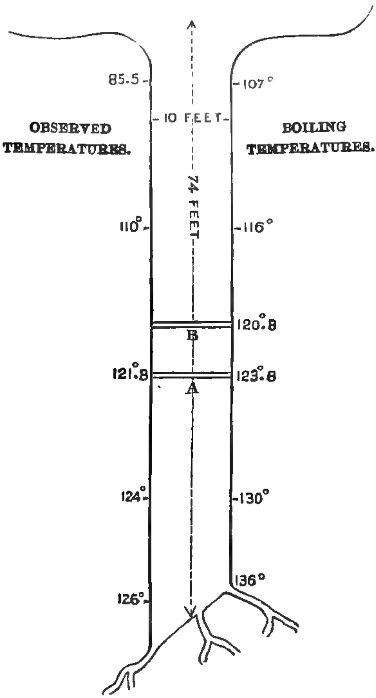
If we place a quantity of the geyser water in an evaporating basin, the following takes place: In the centre of the basin the liquid deposits nothing, but at the sides, where it is drawn up by capillary attraction, and thus subjected to speedy evaporation, we find a ring of silica deposited. Not until the evaporation has continued a considerable time is the slightest turbidity found in the middle of the water. This experiment is the microscopic representant of what occurs in Iceland. Imagine the case of a simple thermal siliceous spring, whose waters trickle

down a gentle incline; the water thus exposed evaporates, and silica is deposited. This deposit gradually elevates the side over which the water passes, until, finally, the latter has to take another course. The process is repeated here, the ground being elevated as before, and the spring has again to move forward. Thus it is compelled to travel round and round, depositing its silica and deepening the shaft in which it dwells, until finally, in the course of ages, the simple spring has produced that wonderful apparatus which so long puzzled and astonished both the tourist and the philosopher.

Previous to an eruption, both the tube and basin are filled with hot water: detonations which shake the ground are heard at intervals, and each explosion is succeeded by a violent agitation of the water in the basin. The water column is lifted up, forming an eminence in the middle of the basin, and an overflow is the consequence. These detonations are evidently due to the production of steam in the ducts which feed the geyser tube, which steam, rushing into the cooler water of the tube, is there suddenly condensed, and produces the noise. In 1846 Professor Bunsen succeeded in determining, a few minutes before a great eruption, the temperature of the geyser tube, from top to bottom; and these observations revealed the extraordinary fact, that at no part of the tube did the water reach its boiling point. In the annexed sketch (fig. 53) I have given, on one side, the temperatures actually observed, and on the other side the temperatures at which water would boil, taking into account both the pressure of the atmosphere and of the superincumbent column of water. The nearest approach to the boiling point is at A, thirty feet from the bottom; but even here the water is 2° Centigrade, or more than $3\frac{1}{2}$° Fahr., below the temperature at which it could boil. How then is it possible that an eruption could occur under such circumstances?

Fix your attention upon the water at the point A, where the temperature is within 2° C. of the boiling point. Call to mind the lifting of the column when the detonations are heard. Let us suppose that by the entrance of steam from the ducts near the bottom of the tube, the

FIG. 53.



geyser column is elevated six feet, a height quite within the limits of actual observation; the water at A is thereby transferred to B. Its boiling point at A is 123·8°, and its actual temperature 121·8°; but at B its boiling point is only 120·8°; hence, when transferred from A to B, the heat which it possesses is in excess of that necessary to make it boil. This excess of heat is instantly applied to

the generation of steam: the column is lifted higher, and the water below is further relieved. More steam is generated, and from the middle downwards the mass suddenly bursts into ebullition. The water above, mixed with steam-clouds, is projected into the atmosphere, and we have the geyser eruption in all its grandeur.

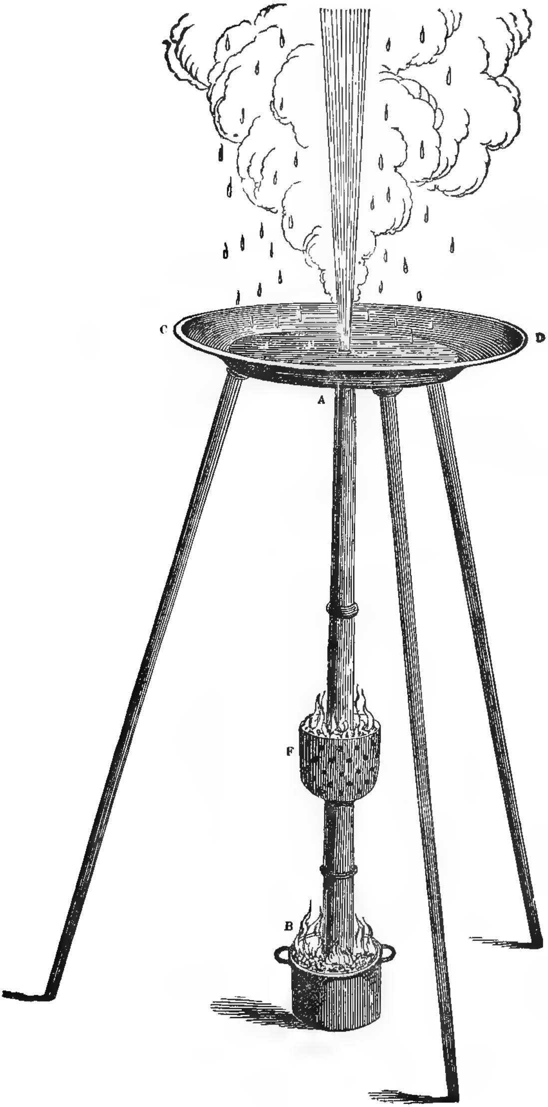
By its contact with the air the water is cooled, falls back into the basin, partially refills the tube, in which it gradually rises, and finally fills the basin as before. Detonations are heard at intervals, and risings of the water in the basin. These are so many futile attempts at an eruption, for not until the water in the tube comes sufficiently near its boiling temperature to make the lifting of the column effective, can we have a true eruption.

To the illustrious Bunsen we owe this beautiful theory, and now let us try to justify it by experiment.[1] Here is a tube of galvanised iron, six feet long, A B (fig. 54), surmounted by a basin, C D. It tapers from a diameter of 6 inches at the bottom to a diameter of 1⅝ inch at the top. It is heated by a fire underneath; and, to imitate as far as possible the condition of the geyser, the tube is encircled by a second fire, F, at a height of two feet from the bottom. Doubtless the high temperature of the water, at the corresponding part of the geyser tube, is due to a local action of the heated rocks. The tube is filled with water, which gradually becomes heated to the boiling temperature; and regularly, every five minutes afterwards, the liquid is ejected into the atmosphere.

There is another famous spring in Iceland called the Strokkur, which is usually forced to explode by stopping its mouth with clods. We can imitate the action of this spring by stopping the mouth of our tube A B—not too tightly be it observed—with a cork. The heating pro-

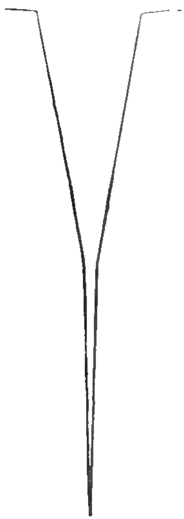
[1] The first artificial geyser was, I believe, constructed by the late Dr. Bromeis of Marburg.

Fig. 54.



gresses. The steam finally attains sufficient tension to eject the cork, and the water, suddenly relieved from the pressure, bursts forth into the atmosphere. The ceiling of this room is nearly thirty feet from the floor, but the eruption has reached the ceiling, from which the water now drips plentifully. In fig. 55 is given a section of the Strokkur.

FIG. 55.



By stopping our model geyser tube with corks, through which glass tubes of various lengths and diameters pass, the action of many of the other eruptive springs of Iceland may be accurately imitated. We can readily, for example, produce an intermittent action; discharges of water and impetuous steam-gushes following each other in quick succession, the water being squirted in jets fifteen or twenty feet high. These experiments completely verify the theory of Bunsen, and we are relieved from the necessity of imagining underground caverns and syphons, filled with water and steam, which were formerly regarded as necessary to the production of these wonderful phenomena.

A moment's reflection will suggest to you that there must be a limit to the operations of the geyser. When the tube has reached such an altitude that the water in the depths below, owing to the increased pressure, cannot attain its boiling point, the eruptions of necessity cease. The spring, however, continues to deposit its silica, and often forms a *Laug*, or cistern. Some of those in Iceland are forty feet deep, and their beauty, according to Bunsen, is indescribable. Over the surface curls a light vapour, the water is of the purest azure, and tints with its own hue the

fantastic incrustations on the cistern walls; while, at the bottom, is often seen the mouth of the once mighty geyser. There are in Iceland traces of vast, but now extinct, geyser operations. Mounds are observed, whose shafts are filled with rubbish, the water having forced a passage underneath and retired to other scenes of action. We have, in fact, the geyser in its youth, manhood, old age, and death, here presented to us. In its youth, as a simple thermal spring; in its manhood, as the eruptive column; in its old age, as the tranquil *Laug*; while its death is recorded by the ruined shaft and forsaken mound, which testify the fact of its once active existence.

LECTURE VII.

ORIGIN OF THE IDEA OF THE CONSERVATION OF FORCE—EXPLANATION AND DEFINITION OF ENERGY: POTENTIAL AND DYNAMIC ENERGY—ENERGY OF MASSES AND OF MOLECULES—SPECIFIC AND LATENT HEAT—EXPERIMENTAL ILLUSTRATIONS—MECHANICAL VALUES OF THE ACTS OF COMBINATION, CONDENSATION, AND CONGELATION IN THE CASE OF WATER—SOLID CARBONIC ACID—THE SPHEROIDAL STATE OF LIQUIDS—FREEZING OF WATER AND MERCURY IN A RED-HOT CRUCIBLE.

ELASTIC AND INELASTIC COLLISION.

IT was formerly universally supposed that by the collision of unelastic bodies force was destroyed. Men saw, for example, when two balls of clay, or painter's putty, or lead, were urged together, that the motion possessed by the masses prior to impact was more or less annihilated. They believed in an absolute destruction of the force of impact. In the collision of elastic bodies, on the contrary, it was observed that the motion with which they clashed together was in great part restored by the resiliency of the masses, the more perfect the elasticity the more complete being the restitution. This led to the notion of *perfectly* elastic bodies—bodies competent to restore by their recoil the whole of the motion which they possessed before impact. Hence arose the idea of the *conservation* of force, as opposed to the destruction of force, which was supposed to occur when inelastic bodies met in collision. We now know that the principle of conservation holds equally good with elastic and inelastic bodies. Perfectly elastic bodies, if such existed, would develop no heat on

collision. They would retain the whole of their motion afterwards, though its direction might be changed. It is only when sensible motion is, in whole or in part, destroyed that the motion of heat is generated, the heat being the exact equivalent of the lost molar motion. Leibnitz expressly affirmed that in inelastic collision no force is really lost.[1]

VIS VIVA.

This subject is now ready for further development. The simplest form of work is the raising of a weight. A man walking up-hill, or up-stairs, carrying a pound weight in his hand, to an elevation say of sixteen feet, performs a certain amount of work over and above the lifting of his own body. If he ascend to a height of thirty-two feet, he does twice the work; if to a height of forty-eight feet, he does three times the work; if to sixty four feet, he does four times the work, and so on. If, moreover, he carries up two pounds instead of one, other things being equal, he does twice the work; if three, four, or five pounds, he does three, four, or five times the work. In fact, it is plain that the work performed depends on two factors, the weight raised and the height to which it is raised. It is expressed by the product of these two factors.

But a body may be caused to reach a certain elevation in opposition to gravity, without being actually carried up. If a hodman, for example, wished to land a brick at an elevation of sixteen feet above the place where he stands, he would probably pitch it up to the bricklayer. He would thus impart, by a sudden effort, a velocity to the

[1] Dr. Berthold draws attention to a wonderfully happy image employed by Leibnitz. He compared the passage of molar into molecular motion to the conversion of a large piece of money into small change. *Berichte d. Preus. Akad.* 1875, p. 584.

brick sufficient to raise it to the required height; the work accomplished by that effort being precisely the same as if he had slowly carried up the brick. The initial velocity to be imparted in the case here assumed is well known. To reach a height of sixteen feet, the brick must quit the man's hand with a velocity of thirty-two feet a second. It is needless to say that a body starting with any velocity would, if wholly unopposed or unaided, continue to move for ever with the same velocity. But when, as in the case before us, the body is thrown upwards, it moves in opposition to gravity, which incessantly retards its motion, and finally brings it to rest at an elevation of sixteen feet. If not here caught by the bricklayer, it would return to the hodman with an accelerated motion, and reach him with the precise velocity it possessed on quitting his hand.

Supposing the man competent to impart to the brick, at starting, a speed of sixty-four feet a second, or twice its former speed, would the amount of work performed in this effort be only twice what it was in the first instance? Our fifth lecture will have prepared us for a negative answer. It would not be twice, but four times that quantity. The height attained, or the work done, is not proportional to the velocity, but to the *square* of the velocity. As before, the work is also proportional to the weight raised. Hence the work which any moving mass whatever is competent to perform, by the motion which at any moment it possesses, is jointly proportional to the weight and the square of the velocity. Here, then, we have a second measure of work, in which we simply translate the idea of height into its equivalent idea of motion. In mechanics, half the product of the mass of a moving body into the square of its velocity, expresses what is called its *vis viva*, which Leibnitz held to be the true measure of force.

ENERGY.

Broadly enunciated, the principle of the conservation of force asserts that the quantity of force in the universe is as unalterable as the quantity of matter; that it is alike impossible to create force and to annihilate it. But in what sense are we to understand this assertion? It would be manifestly inapplicable to the force of gravity as Newton defined it; for this is a force varying inversely as the square of the distance, and to affirm the constancy of a varying force would be self-contradictory. This was a difficulty with Faraday. Yet, when the question is properly understood, gravity forms no exception to the law of conservation.

This lead weight, which we have already employed, now rests upon the earth, motion by their mutual approach being no longer possible. As far as the attraction of gravity is concerned, the possibility of producing motion or performing work ceases as soon as the attracting bodies are in contact. By means of a pulley and string I now draw this weight to a height of sixteen feet above the floor. It remains there just as motionless as when it rested on the floor; but, by the introduction of a space between the floor and it, the conditions are entirely changed. There is now an action possible to the weight which was not possible when it rested upon the earth; *it can fall*, and in its descent can turn a machine, or perform other work. Or going farther away, let us consider the case of a small asteroid placed at a distance of say 8,000 miles from the earth's centre. Its attraction at this distance, which we may suppose determined by a spring balance, would be only one-fourth of its attraction at the earth's surface. If free to do so, it would move towards the earth with a continually accelerated velocity. A continuous pull would be exerted on it from the moment of

its starting, the pull growing stronger as it approached the earth. Now this long continuous pull may be regarded as made up of an infinite number of momentary pulls, the sum of which would be the total effort, if I may use the term, exerted by the earth upon the asteroid during its period of translation.

It is customary in mechanics to represent the magnitude of a force by a line of a certain length, a force of double magnitude being represented by a line of double length, and so on. We can in imagination draw a straight line from the starting point of the asteroid to the earth in the direction of its centre, and erect at every point of that line a perpendicular proportional in length to the attraction exerted at that point. We should thus obtain an infinite number of perpendiculars of gradually increasing length as we approach the earth. Uniting the ends of all these perpendiculars, we should obtain a curve, and between this curve and the straight line joining the asteroid and the earth we should have an area embracing all the perpendiculars placed side by side. Each one of this infinite series of perpendiculars representing an attraction, acting for an infinitely small time, the area just referred to would represent the total effort capable of being exerted upon the asteroid during its passage to the earth from its first position.

Up to the present point we have been dealing with attractions only, the idea of *vis viva* being entirely foreign to our contemplation. But let the asteroid begin to move in obedience to the pull. Motion being once set up, the idea of *vis viva* arises. In moving towards the earth the asteroid consumes, as it were, the attractions. Let us fix our attention on it at any point of its path. Between that point and the earth there is a store of unused attractions—of unexerted pulls. Beyond that point the attractions have been all consumed, and we have in their

place an equivalent quantity of *vis viva.* After the asteroid has passed any point, the attraction previously in store at that point disappears, but not without having added, during the infinitely small duration of its action, a due amount of motion to that previously existing. The nearer the asteroid approaches to the earth, the smaller is the sum of the attractions remaining, but the greater is the *vis viva*; the farther the asteroid, the greater is the sum of the unconsumed attractions, and the less is the living force. Now the principle of conservation affirms *not* the constancy of the attractions, nor yet the constancy of the *vis viva*, but the constancy of their joint value as work producers. At the beginning the *vis viva* was zero and the attraction-area a maximum; close to the earth the *vis viva* is a maximum, while the tension-area is zero. At every other point the work-producing power of the asteroid consists in part of *vis viva* and in part of attractions not yet consumed.

I have thus far tried to steer clear of confusion by fixing your minds upon things rather than upon names. But good names are essential; and here, as yet, we are not provided with such. We have had the force of gravity and living force—two utterly distinct things; and we might have had the force of heat, the force of light, the force of magnetism, or the force of electricity—all of which terms have been employed more or less loosely by writers on physics. This confusion in the use of the word 'force' is happily avoided by the introduction of the term 'energy,' embracing under it both attraction at a distance and *vis viva.* Energy is possessed by bodies already in motion; it is then actual, and we agree to call it *actual* or *dynamic energy.* On the other hand, energy is possible to mutually attracting bodies not in motion, when distances intervenes. They possess a power of motion which would realise itself if all hindrances were

removed. Energy is possible to such bodies, and we agree to call this *potential energy*. We, moreover, speak of the *conservation of energy* instead of the conservation of force, and say that the sum of the potential and dynamic energies of the material universe is a constant quantity.

ENERGY OF MOLECULAR POSITION. SPECIFIC HEAT.

We must, as usual, turn these conceptions, regarding sensible masses, to account, in forming conceptions regarding insensible masses. As an intellectual act, it is quite as easy to conceive the separation of two mutually attracting *atoms*, as to conceive the separation of the earth and our lead weight. If that weight had been lifted by a steam-engine, an amount of heat equivalent to the work done would have been consumed. And if the force of gravity were far greater than it is, a far greater amount of heat would be expended in the lifting of the weight. Now the atoms of bodies, though we cannot suppose them to be in contact, exert enormous attractions. It would require an almost incredible amount of ordinary mechanical force to augment the distances intervening between the atoms of any solid or liquid, so as to increase its volume in any sensible degree. It would also require a force of great magnitude to squeeze the particles of a liquid or a solid together, so as to make the body sensibly less in size. I have vainly tried to augment permanently the density of a soft metal by pressure. Water, which yields so freely to the hand plunged in it, was for a long time regarded as absolutely incompressible. Great force was brought to bear upon it; but sooner than shrink, it oozed through the pores of the metal sphere which contained it, and spread like a dew on the surface. This is a classical experiment which was long ascribed to an erroneous source. Bacon is its author. About half a century after him a

similar experiment was described by the Secretary of the Accademia del Cimento, and it thus came to be called 'the Florentine Experiment.'[1] Bacon's own account of his experiment is this: 'Now it is certain that rarer bodies (such as air) allow a considerable degree of contraction, as has been stated; but that tangible bodies (such as water) suffer compression with much greater difficulty and to a less extent. How far they do suffer it, I have investigated, in the following experiment: I had a hollow globe of lead made capable of holding about two pints, and sufficiently thick to bear considerable force; having made a hole in it, I filled it with water, and then stopped up the hole with melted lead, so that the globe became quite solid. I then flattened the two opposite sides of the globe with a heavy hammer, by which the water was necessarily contracted into less space, a sphere being the figure of largest capacity; and when the hammer had no more effect in making the water shrink, I made use of a mill or press; till the water, impatient of further pressure, exuded through the solid lead like a fine dew. I then computed the space lost by the compression, and concluded that this was the extent of compression which the water had suffered, but only when constrained by great violence.'

By refined and powerful means we can now compress water, but the force necessary to accomplish this is very great. When, therefore, we wish to overcome molecular forces, we must attack them by their peers. Heat accomplishes what mechanical energy, as usually wielded, is incompetent to perform. Bodies, when heated, expand, and to effect this expansion the molecular attractions must be overcome; and where the attractions to be surmounted are so vast, we may infer that the quantity of heat necessary to overpower them will be commensurate.

[1] This was pointed out by Mr. Leslie Ellis. To my excellent friend Mr. James Spedding I am indebted for the quotation.

And now I must ask your entire attention. Suppose a certain amount of heat to be imparted to this lump of lead, how is that heat disposed of within the substance? It is applied to two distinct purposes—it performs two different kinds of molecular work. One portion of it excites that species of motion which augments the temperature of the lead, and which is sensible to the thermometer; but another portion of it goes to force the atoms of the lead into new positions, and this portion *is lost as heat*. The pushing asunder of the atoms of the lead in this case, in opposition to their mutual attractions, is exactly analogous to the raising of our weight in opposition to the force of gravity—a loss of heat, in both cases, is the result. Let me try to make the comparison between the two actions still more strict. Suppose that a definite amount of energy is to be expended upon our weight, and that this energy is divided into two portions, one of which is devoted to the actual raising of the weight, while the other is employed to cause the weight, as it rises, to oscillate like a pendulum, we have then the analogue of that which occurs when heat is imparted to the lead. The atoms are pushed apart, but, during their recession, they are caused to vibrate. Thus, the heat communicated to the lead, resolves itself, in part, into atomic potential energy, and in part into the actual energy of vibration, the latter part alone being competent to act upon our thermometers or to affect our nerves.

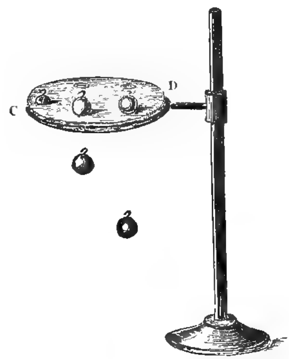
In this case, then, the heat not only imparts actual energy to the vibrating atoms, but also accomplishes what we may call *interior work*.[1] It performs work within the body, by forcing the atoms to take up new positions. When the body cools, the forces which were overcome in the process of heating come again into play

[1] See the excellent memoirs of Clausius in the Philosophical Magazine

the heat which was consumed in the recession of the atoms being restored upon their approach.

The sum of the actual and potential atomic energy—in other words, the total heat—varies widely in bodies of the same weight and temperature. Let me illustrate, by a simple experiment, the differences which exist between bodies, as to the quantity of heat which they are able to take up. Into a vessel containing oil, which is now at a temperature of 180° C., I plunge a number of balls of different metals—iron, lead, bismuth, tin, and copper. At present they all possess the same temperature, namely, that of the oil. I lift them out of the oil, and place them upon a cake of wax C D (fig. 56), which is supported by the ring of a retort-stand. They melt the wax underneath, and sink into it. But they are sinking with different velocities. The iron and the copper are working themselves much more vigorously into the fusible mass than the others; the tin comes next, while the lead and the bismuth lag entirely behind. And now the iron has gone clean through; the copper follows; the bottom of the tin ball just protrudes from the lower surface of the cake, but it cannot go farther; while the lead and bismuth have made but little way, being unable to sink to much more than half the depth of the wax.

FIG. 56.



If, then, equal weights of different substances were all heated, say to 100°, and if the exact amount of heat which each of them gives out in cooling from 100° to 0° were determined, we should find very different amounts

of heat for the different substances. Eminent men have solved this problem, by observing the *times* which different bodies require to cool. Of course, the greater the amount of heat possessed or generated by its atoms, the longer would the body take to cool. The problem has also been solved by plunging different bodies, when heated to a common temperature, into cold water, and observing the gain of heat on the one hand and the loss on the other. It has also been solved, by observing the quantities of ice which different bodies can liquefy, in falling from 100° C. to 0°, or from 212° Fahr. to 32°. These different methods have given concordant results. According to the illustrious French experimenter Regnault, the following numbers express the relative amounts of heat given out by a unit of weight of each of the substances named in the table, in cooling from 98° C. to 15° C.

Aluminium	0·2143	Nickel	0·1086
Antimony	0·0508	Osmium	0·0311
Arsenic	0·0814	Palladium	0·0593
Bismuth	0·0308	Phosphorus (solid)	0·1887
Boron	0·2352	,, (amorphous)	0·1700
Bromine	0·1129	Platinum	0·0329
Cadmium	0·0567	Potassium	0·1696
Carbon	0·2414	Rhodium	0·0580
Cobalt	0·1067	Selenium	0.0827
Copper	0·0952	Silicon	0·1774
Diamond	0·1469	Silver	0·0570
Gold	0·0324	Sodium	0·2934
Iodine	0·0541	Sulphur (native)	0·1776
Iridium	0·0326	,, (recently melted)	0·2026
Iron	0·1138	Tellurium	0·0474
Lead	0·0314	Thallium	0·0336
Lithium	0·9408	Tin	0·0562
Magnesium	0·2499	Tungsten	0·0334
Manganese	0·1217	Water	1·0080
Mercury	0·0333	Zinc	0·0955

A moment's inspection of this table explains the reason why the iron and copper passed through our cake of wax, while

the lead and bismuth were incompetent to do so. It will also be seen that tin, lead, and bismuth occupy the positions which our experiment assigns to them; water, we see, yields more heat than any other substance in the list.

Each of these numbers denotes what has been hitherto called the 'specific heat,' or the 'capacity for heat,' of the substance to which it is attached. As stated on a former occasion, those who considered heat to be a fluid, explained these differences by saying that some substances had a greater store of this fluid than others. We may, without harm, continue to use the term 'specific heat' or 'capacity for heat,' now that we know the true nature of the actions denoted by the term. It is a noteworthy fact, that as the specific heat increases, the *atomic weight* diminishes, and *vice versa*; so that the *product* of the atomic weight and specific heat is, in almost all cases, a sensibly constant quantity.

As measured by any ordinary mechanical standard, the magnitude of the forces engaged in this atomic motion and interior work is enormous. A pound of iron, on being heated from 0° C. to 100° C., expands by about $\frac{1}{300}$th of the volume which it possesses at 0°. Its augmentation would certainly escape the most acute eye; still, to give its atoms the motions corresponding to this increase of temperature, and to shift them through the small space indicated, an amount of heat is requisite which would raise a weight of about eight tons one foot high. The force of gravity almost vanishes in comparison with these molecular forces; the pull of the earth upon our pound weight, as a mass, is as nothing compared with the mutual pull of its own atoms. Water furnishes a still subtler example. Water, as we know, expands on both sides of 4° C. or 39° F. At 4° C. it has its maximum density. Suppose a pound of water to be heated from $3\frac{1}{2}$° C. to $4\frac{1}{2}$°

C.—that is, one degree—its volume at both temperatures is the same; there has been no forcing asunder whatever of the atomic centres, and still, though the volume is unchanged, an amount of heat has been imparted to the water, sufficient, if mechanically applied, to raise a weight of 1,390 lbs. a foot high. As I have already tried to explain to you, the interior work, done here by the heat, is simply that of causing the molecules of water to rotate.

We thus see that there are descriptions of interior work, different from that of pushing the atoms more widely apart. An enormous quantity of interior work may be accomplished, while the atomic centres, instead of being pushed apart, approach each other. Polar forces—forces emanating from distinct atomic points, and acting in distinct directions, give to crystals their symmetry; and the overcoming of these forces, while it necessitates a consumption of heat, may also be accompanied by a diminution of volume. This is illustrated by the deportment of both ice and bismuth in liquefying.

Chemists have determined the relative weights of the atoms of different substances. Calling the weight of a hydrogen atom 1, the weight of an oxygen atom is 16. Hence, to make up a pound weight of hydrogen, sixteen times the number of atoms contained in a pound of oxygen would be necessary. The number of atoms required to make up a pound is, evidently, inversely proportional to the atomic weight. We here approach a very delicate and important point. The experiments of Dulong and Petit, and of Regnault and Neumann, render it extremely probable that all elementary atoms, great or small, light or heavy, when at the same temperature, possess the same amount of the energy we call heat, the lighter atoms making good in velocity what they want in mass. Thus, each atom of hydrogen has the same moving

energy as an atom of oxygen, at the same temperature. But, inasmuch as a pound weight of hydrogen contains sixteen times the number of atoms, it must also contain sixteen times the amount of energy possessed by a pound of oxygen, at the same temperature.

From this it would follow, that to raise a pound of hydrogen a certain number of degrees in temperature—say from 50° to 60°—requires sixteen times the amount of heat needed to raise the temperature of a pound of oxygen the same number of degrees. Conversely, a pound of hydrogen, in falling through 10°, would yield sixteen times the amount of heat yielded by a pound of oxygen falling through the same number of degrees. The atomic weight of nitrogen being 14, this reasoning leads to the conclusion, that a pound of hydrogen contains fourteen times the amount of heat contained by a pound of nitrogen at the same temperature. These conclusions, we shall immediately learn, are verified by experiment.

The most important experiments on the specific heat of gases we owe to Regnault. He determined the quantities of heat necessary to raise equal weights of gases and vapours, and also the quantities necessary to raise equal volumes of them, through the same number of degrees. Calling the specific heat of water 1, the following are the specific heats of some gaseous bodies:

SIMPLE GASES.

	Specific heats	
	Equal weights	Equal volumes
Air	0·237	
Oxygen	0·218	0·240
Nitrogen	0·244	0·237
Hydrogen	3·409	0·236
Chlorine	0·121	0·296
Bromine	0·055	0·304

Equal volumes of all these gases contain the same number of atoms, and hence we should infer that the

specific heats of equal volumes ought to be equal. They are very nearly so for oxygen, nitrogen, and hydrogen; but chlorine and bromine differ considerably from the other elementary gases. Now bromine is a *vapour*, while chlorine is a gas easily liquefied by pressure; hence, in both these cases, the mutual attraction of the atoms, or, in other words, the interior work, which is insensible in oxygen, nitrogen, and hydrogen, demands the expenditure of heat. The specific heats of chlorine and bromine at equal volumes are, therefore, higher than the others.

The specific heat of hydrogen at constant volume is a little less than that of oxygen or nitrogen because it is the most perfect gas. Now the weight of equal volumes of hydrogen and oxygen are to each other as 1 : 16. Hence the heat contained in $\frac{1}{16}$th of a gramme of hydrogen is, in round numbers, equal to that contained in a whole gramme of oxygen. Or, comparing equal weights instead of equal volumes, the heat contained in a gramme of hydrogen is, as indicated above, 16 times that contained in a gramme of oxygen. To nitrogen similar reasoning applies.

Certain simple gases unite to form compound ones, without any change of volume. Thus, one volume of chlorine combines with one volume of hydrogen, to form *two* volumes of hydrochloric acid. In other cases the act of combination is accompanied by a diminution of volume; thus, two volumes of nitrogen combine with one of oxygen to form two volumes of the protoxide of nitrogen. By the act of combination, three volumes have, in this case, been condensed to two. Regnault found that the compound gases formed without condensation have, at equal volumes, nearly the same specific heat as oxygen, nitrogen, and hydrogen; while those which change the volume vary from this rule.

COMPOUND GASES—WITHOUT CONDENSATION.

	Specific heats	
	Equal weights	Equal volumes
Nitric oxide	0·232	0·241
Carbonic oxide	0·245	0·237
Hydrochloric acid	0·185	0·235

The specific heat of equal volumes of these compound gases is sensibly the same as that of the three simple gases already mentioned.

COMPOUND GASES—3 VOLUMES CONDENSED TO 2.

	Specific heats	
	Equal weights	Equal volumes
Carbonic acid	0·217	0·331
Nitrous oxide	0·226	0·345
Aqueous vapour	0·480	0·299
Sulphurous acid	0·154	0.341
Sulphide of hydrogen	0·243	0·286
Bisulphide of carbon	0·157	0·412

Here the specific heats of equal volumes are neither equal to those of the elementary gases, nor to each other. It is worth bearing in mind that the specific heat of water is about double that of aqueous vapour, and also double that of ice. The latter fact brings out the philosophy of Davy's experiment referred to in our second Lecture.

Comparing *equal weights*, the specific heat of water being 1, that of air is 0·237. Hence, a pound of water, in losing one degree of temperature, would warm about 4·2 lbs. of air one degree. But water is 770 times heavier than air; hence, comparing *equal volumes*, a cubic foot of water, in losing one degree of temperature, would raise $770 \times 4{\cdot}2 = 3{,}234$ cubic feet of air one degree.

The vast influence which the ocean must exert, as a moderator of climate, here suggests itself. The heat of summer is stored up in the ocean, and slowly given out during the winter. This is one cause of the absence of

extremes in an island climate. The summer of the island can never attain the fervid heat of the continental summer, nor can the winter of the island be so severe as the continental winter. In various parts of the Continent fruits grow which our summers cannot ripen; but in these same parts our evergreens are unknown; they cannot live through the winters. Winter in Iceland is, as a general rule, milder than in Lombardy.

LATENT HEAT.

We have hitherto confined our attention to the heat consumed in the molecular changes of solid and liquid bodies, while they continue solid and liquid. We shall now direct our attention to the phenomena which accompany changes of the state of aggregation. When sufficiently heated, most solids become liquids; and when still further heated, the liquids become gases. Let us consider the case of ice, and follow it through the entire cycle of its changes. Take then a block of ice at a temperature of 10° C. below zero. The ice being gradually warmed, a thermometer fixed in it rises to 0°. At this point the ice begins to melt, and the thermometric column, which rose previously, *becomes perfectly stationary.* The warmth is still applied, but no augmentation of temperature is shown by the thermometer; and not until the last film of ice has been removed from the bulb, does the mercury resume its motion. It then ascends again, reaching in succession 30°, 60°, 100°. Here steam bubbles appear in the liquid; it boils, and from this point, onwards, the thermometer remains stationary at 100°.

To simply liquefy a pound of ice, as much heat is expended as would raise a pound of water 79·4° C., or 79·4 pounds of water 1° in temperature; while to

convert a pound of water at 100° C. into a pound of steam of the same temperature, 537·2 times as much heat is required as would raise a pound of water one degree in temperature. The former number, 79·4° C. (or 143° F.), represents what has been hitherto called the latent heat of water; and the latter number, 537·2° C. (or 967° F.), represents the latent heat of steam. It was manifest to those who first used these terms, that throughout the entire time of melting, and throughout the entire time of boiling, heat was communicated; but inasmuch as this heat was not revealed by the thermometer, it was said to be rendered *latent*. The fluid of heat was supposed to hide itself, in some unknown way, in the interstitial spaces of the water and the steam.[1] According to our present theory, the heat expended in melting is consumed in overcoming molecular attractions, and in conferring potential energy upon the separated molecules, or their poles. It is, virtually, the lifting of a weight. So, likewise, as regards vaporising, the heat is consumed in separating the molecules still farther asunder, and in conferring upon them a further amount of potential energy. When the heat is withdrawn, the vapour condenses, that is to say the molecules fall together with an energy equal to that employed to separate them. By further chilling, the water crystallises to ice, restitution being made in both cases of the precise quantity of heat consumed in the acts of fusion and vaporisation.

The act of liquefaction consists almost solely of interior work—of work expended in moving the atoms into new positions. The act of vaporisation is also, for the most part, interior work; to which, however, must be added the exterior work of forcing back the atmosphere, when the liquid becomes vapour.

[1] Cavendish rejected the term 'latent heat,' and considered the heat of condensation to be *generated* (see Maxwell, Theory of Heat).

Rumford estimated the calorific power of a body by the number of parts, by weight, of water, which one part, by weight, of the body would, on perfect combustion, raise one degree in temperature. Thus, one pound of charcoal, in combining with $2\frac{2}{3}$ lbs. of oxygen, to form carbonic acid, evolves heat sufficient to raise the temperature of about 8,000 lbs. of water 1° C. Similarly, one pound of hydrogen, in combining with eight pounds of oxygen, to form water, generates an amount of heat sufficient to raise 34,000 lbs. of water 1° C. The calorific powers, therefore, of carbon and hydrogen are as 8 : 34.[1] The refined researches of Favre and Silbermann entirely confirm these determinations of Rumford.

A moment's further attention devoted to this wonderful substance, water, will repay our pains. First, we have its constituents as free atoms of oxygen and hydrogen, which attract each other and combine. The mechanical value of this atomic act is easily determined. The heating of 1 lb. of water 1° C. is equivalent to 1,390 foot-pounds; hence the heating of 34,000 lbs. of water 1° C. is equivalent to $34{,}000 \times 1{,}390$ foot-pounds. We thus find that the concussion of our 1 lb. of hydrogen with 8 lbs. of oxygen is equal, in mechanical value, to the raising of forty-seven million pounds one foot high. It was no over-statement then, on my part, when I affirmed that the force of gravity, as exerted near the earth, is almost a vanishing quantity, in comparison with these molecular forces. The distances which separate the atoms before combination are so small as to be utterly immeasurable; still, it is in passing over these distances that they acquire a velocity, sufficient to cause them to clash with the tremendous energy here indicated.

After combination, the substance is in a state of vapour, which sinks to 100° C., and afterwards condenses

[1] Percy's Metallurgy.

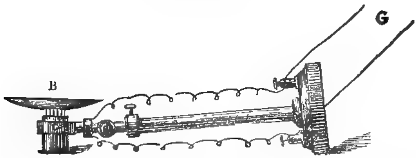
to water. In the first instance, the atoms fall together to form the compound; in the next instance, the molecules of the compound fall together to form a liquid. The mechanical value of this act is also easily calculated; 9 lbs. of steam, in falling to water, generate an amount of heat sufficient to raise $537 \cdot 2 \times 9 = 4,835$ lbs. of water 1° C., or $967 \times 9 = 8,703$ lbs. 1° F. Multiplying the former number by 1,390, or the latter by 772, we have, in round numbers, a product of 6,720,000 foot-pounds, as the mechanical value of the mere act of condensation.[1] The next great fall is from the state of water to that of ice, and the mechanical value of this act is equal to 993,564 foot-pounds. Thus, our 9 lbs. of water, at its origin and during its progress, falls down three great precipices: the first fall is equivalent, in energy, to the descent of a ton weight down a precipice 22,320 feet high; the second fall is equal to that of a ton down a precipice 2,900 feet high; and the third is equal to the fall of a ton down a precipice 433 feet high. The stone-avalanches of the Alps are sometimes seen to smoke and thunder down the declivities, with a vehemence almost sufficient to stun the observer, while the snow-flakes descend so softly as not to hurt the fragile spangles of which they are composed; yet to produce, from aqueous vapour, a quantity of that tender material which a child could carry, demands an exertion of energy competent to gather up the shattered blocks of the largest stone-avalanche that I have ever seen, and pitch them to twice the height from which they fell.

A few experimental illustrations of the calorific effects which accompany the change of aggregation will not be

[1] In Rumford's experiments the heat of condensation was included in his estimate of calorific power; deducting the above number from that found for the chemical union of the hydrogen and oxygen, forty millions of foot-pounds would still remain as the mechanical value of the act of combination

out of place here. I place the thermo-electric pile with its back upon the table, and on its naked face a thin silver basin, B (fig. 57), which contains a quantity of water

FIG. 57.



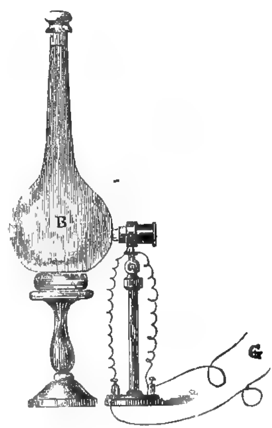
slightly warmed. The needle of the galvanometer swings to 90°, and remains permanently deflected at 70°. I now drop a little powdered nitre, not more than will cover a threepenny-piece, into the basin, and allow it to dissolve. The nitre was previously placed before the fire, so that not only was the liquid warm, but also the solid powder. The effect of their mixture is this. The nitre dissolves in the water; and to produce this change, all the heat which the water and the nitre possess in excess of the temperature of this room, is consumed, and, indeed, a great deal more. The needle not only sinks to zero, but moves strongly up at the other side, showing that the face of the pile is now powerfully chilled.

Pouring out the chilled liquid, and replacing it by tepid water, the permanent deflection of 70° is reproduced. I introduce a pinch of common salt. The needle sinks, reaches zero, and moves up on the side which indicates cold. But the action is not at all so strong as in the case of saltpetre. As regards latent heat, then, we have differences similar to those already illustrated as regards specific heat. Putting a little sugar, instead of salt, into the warm water, the chilling is sensible, but it is much less than in either of the former cases. Thus, when you sweeten your hot tea, you cool it in the most philosophical manner; when you put salt in your soup, you do the

same; and if you were concerned with the act of cooling alone, and careless of the flavour of your soup, you might hasten its refrigeration by adding to it saltpetre.

In our fourth Lecture a mixture of pounded ice and salt was employed to obtain intense cold. Both the salt and the ice, when they are thus mixed together, change their state of aggregation, and, as a consequence, the temperature of the mixture sinks many degrees below the freezing point of water. By this cold we were able to burst our iron bombs. I will now reverse this process, and endeavour to show you the heat developed, in passing from the liquid to the solid state. But first let me prove that when sulphate of soda is dissolved, heat is absorbed. Testing the substance as the nitre was tested, as the crystals of the sulphate melt the pile is chilled. The complementary experiment is here arranged. A large glass vessel, B (fig. 58), with a long neck, is filled with a solution of sulphate of soda. Yesterday the substance was dissolved in a pan over our laboratory fire, and this bottle was filled with the solution. The top being carefully covered with a piece of bladder, the bottle was placed behind this table, where it remained undisturbed throughout the night.

FIG. 58.



The liquid, at the present moment, is super-saturated with sulphate of soda. When the water was hot, it melted more than it could melt when cold; and now the temperature has sunk lower than that which corresponds to the point of saturation. This state of things is secured by keeping the solution perfectly still, and per-

mitting nothing to fall into it. Water, kept thus still, may be cooled many degrees below its freezing point. Some of you may have noticed the water in your jugs, after a cold winter night, partially freeze on being poured out in the morning. In cold climates this is not uncommon. The molecules of sulphate of soda, in this solution, are, as it were, on the brink of a precipice, and may be pushed over it, by simply dropping a small crystal of the substance, not larger than a grain of sand, into the solution. I cut away the bladder and drop the bit of crystal into the clear liquid; it does not sink, the molecules having closed round it to form a solid in which it is now embedded. The passage of the atoms from a state of freedom to a state of bondage goes on quite gradually; you see the solidification extending down the neck of the bottle. The naked face of the thermo-electric pile rests against the convex surface, and the needle of the galvanometer points to zero. The process of crystallisation now approaches the liquid in front of the pile. This solidifies and develops heat, which, communicated to the glass envelope, warms the pile, and the needle flies to 90°. The quantity of heat thus rendered sensible by solidification is exactly equal to that which was rendered latent by liquefaction. The latent heat of liquids is thus illustrated.

Let me now direct your attention to a few experiments illustrative of what has been called the latent heat of vapours. As before, the pile is laid upon its back, with its naked face upwards; on this face is placed the silver basin containing a small quantity of a volatile liquid, purposely warmed. The needle moves, indicating heat. But scarcely has it attained 90°, when it turns promptly, descends to 0°, and flies up with violence on the side of cold. The liquid here used is sulphuric ether; it is very volatile, and the speed of its evaporation is such, that it consumes

rapidly the heat at first communicated to it, and then abstracts heat from the face of the pile. I remove the ether, and supply its place by alcohol, slightly warm; the needle, as before, ascends on the side of heat. By a pair of small bellows we can promote the evaporation of the alcohol; the needle descends, and passes to 90° on the side of cold. Water is not nearly so volatile as alcohol; still, with the aid of the bellows the refrigeration of water may also be readily shown. For chilling water, we sometimes use unglazed pottery, which admits of a slight percolation of the liquid, and thus causes a dewiness on the external surface. From that surface evaporation goes on, and the heat necessary for this molecular work, being drawn in great part from the water within, keeps it cool. Butter-coolers are made on the same principle.

Water may even be frozen, through the simple abstraction of heat by its own vapour. The beautiful instrument

Fig. 59.

which effects this is called the *cryophorus*, or ice-carrier. It was invented by Dr. Wollaston. The shape of the *cryophorus* is shown in fig. 59, and it is thus prepared: A little water is put into the bulb, A; the other bulb, B, while softened by heat, has a tube drawn out from it, with a minute aperture at the end. The water is boiled in A, and the boiling is continued until the steam produced has chased all the air away through the small aperture in the distant bulb. When the bulbs, and connecting tube,

are filled with pure steam, the small orifice of B is sealed with a blowpipe. Here, then, we have water and its vapour, with scarcely a trace of air. You hear how the liquid rings, exactly as it did in the case of the water-hammer.

I turn all the liquid into one bulb, A, which is dipped into an empty drinking glass, covered at the top with paper to protect the bulb from air currents. The empty bulb, B, is plunged into a freezing mixture; thus, the vapour which escapes from the liquid in the bulb A, is condensed, by the cold, in B. This condensation permits of the formation of new vapour. As the evaporation continues, the water which supplies the vapour becomes more and more chilled. In a quarter of an hour, or twenty minutes, it will be converted into a cake of ice. Here, indeed, is the opalescent solid, formed in a second instrument, which was set in action about half an hour ago.

A still more striking example of the consumption of heat, in changing the state of aggregation, is furnished by liquefied carbonic acid, a gallon of which is here imprisoned in a strong iron bottle. The substance, you know, is a gas under ordinary circumstances. When the cock which closes the bottle is turned, the pressure upon the acid is relieved; the liquid boils violently—flashes, as it were, suddenly into gas, which rushes from the orifice with impetuous force. Mixed with the current of gas you see a white substance, which is blown to a distance of eight or ten feet through the air. This is carbonic acid *snow*. The cold produced, in passing from the liquid to the gaseous state, is so intense, that a portion of the carbonic acid is actually frozen to a solid, which mingles, in small flakes, with the issuing stream of gas. The snow may be collected in a cylindrical box, with two perforated handles, through which the gas is allowed to issue. Right and left you see the turbid current, but a large portion of

the frozen mass is retained in the box. On being opened, you see it filled with this perfectly white solid.

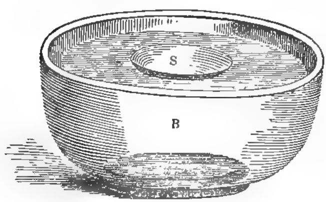
The solid disappears very gradually; its conversion into vapour is slow, because it can only slowly collect from surrounding substances the heat necessary to vaporise it. You can handle it freely, but must not press it too much, lest it should burn you. It is cold enough to burn the hand. When a piece of it is plunged into water, and held there, bubbles are seen rising through the water—these are pure carbonic acid gas. I put a bit of the acid into my mouth, taking care not to inhale while it is there. Breathing against a candle, my breath extinguishes the flame. How it is possible to keep so cold a substance in the mouth without injury will be immediately explained. A piece of iron, of equal coldness, would do serious damage.

Water will not melt this snow, but sulphuric ether will; and on pouring a quantity of the ether on the snow, a pasty mass is obtained, which has an enormous power of refrigeration. Over the bottom of a porcelain basin is spread a little paper, and over the paper is poured a pound or two of mercury; on the mercury I place some solid carbonic acid, and over the acid I pour a little ether. Mercury, you know, requires a very low temperature to freeze it; but here it is rapidly frozen to a solid which can be hammered, and also cut with a knife. By means of a wire frozen into it I raise the mercury, and plunge it into a glass jar containing water. It liquefies, and showers downwards through the water; but every fillet of mercury freezes the water with which it comes into contact, and thus, round each fillet is formed a tube of ice, through which the liquid metal is seen descending.

THE SPHEROIDAL STATE.

I have now to direct your attention to another, and very singular class of phenomena, connected with the production of vapour. On the table is a broad porcelain vessel, B (fig. 60), filled with hot water. Placing a light silver basin, heated to redness, on the hot water, as at S, what will occur? You might naturally reply that the basin will impart its heat instantly to the water, and be cooled down to the temperature of the latter. But nothing of this kind occurs. The silver for a time develops a sufficient amount of vapour underneath it, to lift it entirely out of contact with the water; or, in the language of the hypothesis developed on a former occasion, it is lifted by the discharge of molecular projectiles against its under surface. This will go on, until the temperature of the basin sinks, and it is no longer able to produce vapour of sufficient tension to support it. Then it comes into contact with the water, and the ordinary hissing of a hot metal, together with the cloud which forms overhead, declares the fact.

Fig. 60.

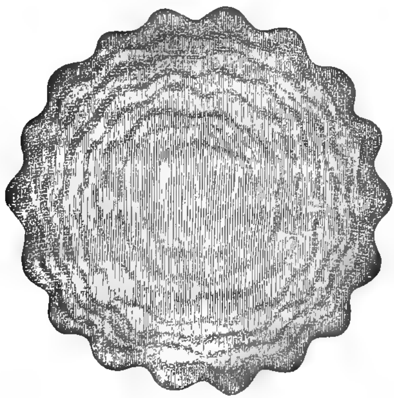


Let us now reverse the experiment, and instead of placing the basin in hot water, place the water in a red-hot basin. You hear no noise of ebullition, no hissing of the water; the drop rolls about on its own vapour—that is to say, it is sustained by the recoil of the molecular projectiles, discharged from its under surface. I withdraw the lamp, and allow the basin to cool, until it is no longer able to produce vapour strong enough to support the drop. The liquid then touches the metal; violent ebullition sets in,

and the cloud, which you now observe, forms above the basin.

You cannot, from your present position, see this flattened spheroid rolling about in the hot basin; but it may be shown to you, and, if we are fortunate, you will see something very beautiful. There is, underneath the drop, an incessant development of vapour, which, as incessantly, escapes from it laterally. If the drop rest upon a flattish surface, so that the lateral escape is very difficult, the

FIG. 61.

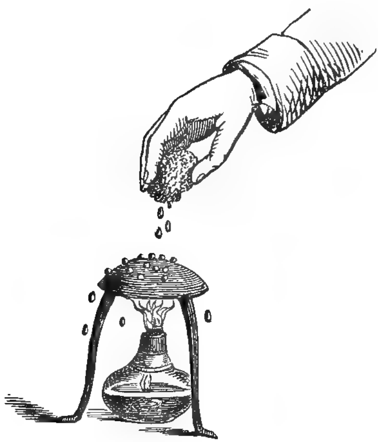


vapour will burst up through the middle of the drop. But matters are here so arranged, that the vapour shall issue laterally; and it sometimes happens that the escape is rhythmic; the vapour issues in regular pulses, and then we have our drop of water moulded to a most beautiful rosette, two inches in diameter. Throwing the beam of the electric lamp upon this drop, and holding a lens over it, I cast its image on the ceiling where it is now perfectly defined, forming a figure (fig. 61) eighteen inches in diameter, with the vapour breaking, as if in music, from its edge. I withdraw the heat; the undulation continues for

some time, diminishing gradually; the border becomes unindented, the drop becomes motionless—a liquid spheroid—and now it suddenly spreads hissing upon the surface, for contact has been established, and the 'spheroidal condition' is at an end.

Placing the silver basin, with its bottom upwards, in front of the electric lamp, by means of a lens an image of the rounded outline of the basin is cast upon the screen. Dipping a bit of sponge into alcohol and

Fig. 62.

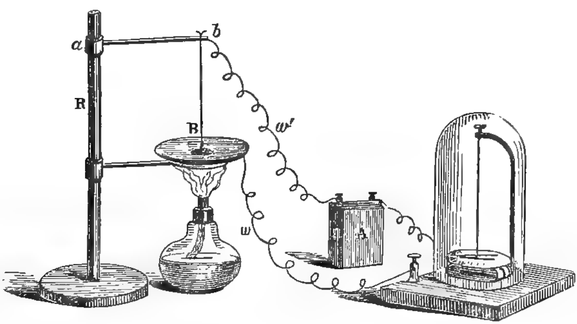


squeezing it over the cold basin, the drops spread out over the convex surface and trickle down it. Let us now heat the basin by placing a lamp underneath it. On squeezing the sponge the drops descend as before, but, when they come in contact with the basin, they no longer spread but roll over the surface as liquid spheres (fig. 62), bounding and dancing, as if they had fallen upon elastic springs.

The arrangement next to be presented to you, which was suggested by the late Professor Poggendorff, shows,

in a very ingenious manner, the interruption of contact between the spheroidal drop and its supporting surface. From a silver basin, B (fig. 63), intended to hold the drop, a wire, *w*, is carried to a galvanometer, the other end of the galvanometer wire being attached to a small battery, A. From the opposite pole of this battery a wire, *w′*, is carried to the movable arm, *a b*, of a retort-stand, from which a platinum wire intended to dip into the spheroidal drop, descends vertically. I heat the basin, pour in the water, and lower the wire till the end of it dips into the

FIG. 63.

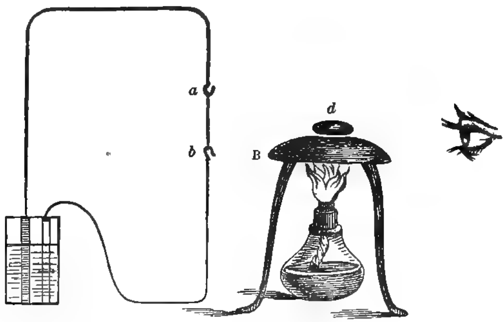


spheroid: you see no motion of the galvanometer needle; still, the only gap in the entire circuit is that now existing underneath the drop. If the drop were in contact, the current would pass. This is proved by withdrawing the lamp; the spheroidal state will soon end; the liquid will touch the bottom. It now does so, and the needle instantly flies aside.

You can actually *see* the interval between the drop and the hot surface upon which it rests. A private experiment may be made in this way: Let a flattish basin, B (fig. 64), be turned upside down, and let the bottom of it be slightly indented, so as to be able to bear a drop. Heat the basin with a spirit lamp, and place upon it a drop of

ink, *d*, with which a little alcohol has been mixed Stretch a platinum wire, *a b*, vertically behind the drop, and render the wire incandescent, by sending a current of electricity through it. Bring your eye to a level with the bottom of the drop, and you will be able to see the red-hot wire, through the interval between the drop and the surface which supports it. This interval, moreover, can readily be made visible to you all, by sending through it the beam of the electric lamp, and casting its image upon the screen.

FIG. 64.



The spheroidal condition was first observed by Leidenfrost. M. Boutigny has lent new interest to this subject by expanding the field of illustration, and applying it to the explanation of many extraordinary effects. If the hand, for example, be wet, it may be passed through a stream of molten metal without injury. A blacksmith will lick a white-hot iron without fear of burning, his tongue being effectually preserved from contact with the iron, by the vapour developed. To the vapour of the carbonic acid, which shielded me from its contact, I owed my safety when holding the substance in my mouth. To the same protective influence, many escapes from the fiery ordeal of ancient times have been attributed. It may be added that the explanation of the spheroidal condition, given by

M. Boutigny, has not been accepted by scientific men. The foregoing experiments reduce its real cause to ocular demonstration.

The spheroidal condition enables us to perform the extraordinary experiment of freezing a liquid in a red-hot vessel. By means of sulphurous acid M. Boutigny first froze water in a red-hot crucible; and Mr. Faraday, by means of solid carbonic acid, subsequently froze mercury. Let us first operate with water. This hollow sphere of brass, now filled with water, is formed of two hemispheres, soldered together. Into the sphere is screwed a wire intended to serve as a handle. Heating a platinum crucible to glowing redness, I place in it some lumps of solid carbonic acid. When ether is poured on the acid, neither of them comes into contact with the hot crucible, being protected from contact by the elastic cushion of vapour which surrounds them. Lowering the sphere of water down upon the mass, I carefully pile fragments of carbonic acid over it, adding also a little ether. The pasty substance, within the red-hot crucible, remains intensely cold. A crack is heard, and you are thereby assured that the experiment has succeeded. The freezing water has burst the brass sphere along the line of solder. Raising the sphere, and peeling off the severed hemispheres, we rescue a solid ball of ice from the red-hot crucible.

Turn we now to mercury. By means of a stout wire handle, I dip a conical copper spoon, containing the liquid metal, into the red-hot crucible, and surround it as before with the carbonic acid and ether. The ether vapour has taken fire, which was not intended. The experiment ought to be so made, that the carbonic acid gas—the choke damp of mines—shall preserve the ether from ignition. The mercury, however, freezes, the presence of the flame adding to the impressiveness of the result, as the intensely cold solid is lifted through the fire.

LECTURE VIII.

CONVECTION OF HEATED AIR—WINDS—THE UPPER AND LOWER 'TRADES'—EFFECT OF THE EARTH'S ROTATION ON THE DIRECTION OF WIND—INFLUENCE OF AQUEOUS VAPOUR UPON CLIMATE—EUROPE THE CONDENSER OF THE WESTERN ATLANTIC—RAINFALL IN IRELAND—THE GULF STREAM—FORMATION OF SNOW—FORMATION OF ICE FROM SNOW—GLACIERS—PHENOMENA OF GLACIER MOTION—REGELATION—MOULDING OF ICE BY PRESSURE—ANCIENT GLACIERS—THEORETIC ERRORS REGARDING THEIR CAUSE.

ATMOSPHERIC CIRCULATION—CONVECTION IN AIR.

I PROPOSE devoting an hour to-day to the consideration of some of the thermal phenomena which occur, on a large scale, in Nature. And first, with regard to winds. Observe those sunburners, intended to illuminate this room, when the daylight is intercepted, or gone. Not to give light alone were they placed there but, in part, to promote ventilation. The air, heated by the gas flames, expands, and issues in a strong vertical current into the outer atmosphere. The air of the room is thereby incessantly drawn upon, and a fresh supply must be introduced to make good the loss. Our chimney draughts are so many vertical winds, due to the heating of the air by our fires.

When a piece of brown paper is ignited, the flame ascends; and when the flame is blown out, the smouldering edges warm the air, and produce currents which carry the smoke upward. I dip the smoking paper into a large glass vessel, and stop the neck to prevent the escape of the smoke; the smoke ascends with the light hot air in

the middle, spreads out laterally above, is cooled, and falls like a cascade of cloud along the sides of the vessel. I have frequently traced the smoke of a brushwood fire to a height of thousands of feet in the Alps. When a poker or a heavy iron spatula, heated to dull redness, is held in the air, you cannot see the currents ascending from it. But they reveal themselves by their action on the rays of light. Placing the poker or spatula in a strong beam, so that its black shadow is thrown upon a white screen, waving lines of light and shade mark the streaming upwards of the heated air. If a fragment of sulphur, contained in an iron spoon, be heated until it ignites, and then plunged into a jar of oxygen, the combustion becomes brilliant and energetic, and the air of the jar is thrown into intense commotion. The fumes of the sulphur enable you to track the storms, which the heating of the air produces within the jar. I use the word 'storms' advisedly, for the hurricanes which desolate the earth are nothing more than large illustrations of the effect produced in the glass jar.

From the heat of the sun our winds are all derived. We live at the bottom of an aërial ocean, in a remarkable degree permeable to the solar rays, and but little disturbed by their direct action. But those rays, when they fall upon the earth, heat its surface, and, when they fall upon the ocean, they provoke evaporation. The air in contact with the surface shares its heat, is expanded, and ascends into the upper regions of the atmosphere, while the vapour from the ocean also ascends, because of its lightness, carrying air along with it. Where the rays fall vertically on the earth, that is to say, between the tropics, the heating of the surface is greatest. Here aërial currents ascend and flow laterally, north and south, towards the poles, the heavier air of the polar regions streaming in to supply the place vacated by the light and

warm air. Thus, we have incessant circulation. A few days ago, in the hot room of a Turkish bath, I held a lighted taper in the open doorway, midway between top and bottom. The flame rose vertically from the taper. When placed at the bottom, the flame was blown violently inwards; when placed at the top, it was blown violently outwards. Here we had two currents, or winds, sliding over each other, and moving in opposite directions. Thus, also, as regards our hemisphere, a current from the equator sets in towards the north, and flows in the higher regions of the atmosphere, while, to supply its place, another flows towards the equator in the lower regions of the atmosphere. These are the upper and the lower Trade Winds.

Were the earth motionless, these two currents would run directly north and south, but the earth rotates from west to east on its axis, once in twenty-four hours. In virtue of this rotation, the air at the equator is carried round with a velocity of 1,000 miles an hour. As we withdraw from the equator, the velocity due to the earth's rotation diminishes, and it becomes nothing at the poles. It is proportional to the radius of the parallel of latitude, and diminishes as these circles diminish in size. You have observed what takes place when a person incautiously steps out of a carriage in motion. He shares the motion of the carriage, and when his feet touch the earth he is thrown forward in the direction of the motion. This is what renders leaping from a railway carriage, when the train is at full speed, generally fatal. Imagine, then, an individual suddenly transferred from the equator to a place where the velocity, due to rotation, is only 900 miles an hour; on touching the earth he would be thrown forward in an easterly direction, with a velocity of 100 miles an hour, this being the difference between the equatorial velocity with which he started, and the velocity of the earth's surface in his new locality.

Similar considerations apply to the transfer of air from the equatorial to the northern regions, and *vice versâ.* At the equator the air possesses the velocity of the earth's surface there, and, on quitting this position, it not only has its tendency northwards to obey, but also an eastward tendency, and it must take a resultant direction. The farther it goes north, the more it is deflected from its original course; the more it turns towards the east, and tends to become what we should call a westerly wind. The opposite holds good for the current proceeding *from* the north; this passes from places of slow motion to places of quick motion: it is met by the earth; hence, the wind which started as a north wind becomes a north-east wind; and, as it approaches the equator, it becomes more and more easterly.

It is not by reasoning alone that we arrive at a knowledge of the existence of the upper atmospheric current, though reasoning is sufficient to show that compensation must take place somehow—that a wind cannot blow in any direction without an equal displacement of air taking place, in the opposite direction. But clouds are sometimes seen in the tropics, high in the atmosphere, moving in a direction opposed to that of the constant wind below. Could we discharge a light body with sufficient force to cause it to penetrate the lower current, and reach the higher, the direction of the body's motion would give us that of the wind above. Human strength cannot perform this experiment, but it has nevertheless been made. Ashes have been shot through the lower current by volcanoes, and, from the places where they have subsequently fallen, the direction of the wind which carried them has been inferred. Professor Dove, who has so enriched the knowledge of the age by his researches in meteorology, cites the following instance: 'On the night of April 30, explosions like those of heavy artillery were heard at

Barbadoes, so that the garrison at Fort St. Anne remained all night under arms. On May 1, at daybreak, the eastern portion of the horizon appeared clear, while the rest of the firmament was covered by a black cloud, which soon extended to the east, quenched the light there, and at length produced a darkness so intense that the windows in the rooms could not be discerned. A shower of ashes descended. Whence came these ashes? From the direction of the wind, we should infer that they came from the Azores; they came, however, from the volcano Morne Garou in St. Vincent, which lies about 100 miles west of Barbadoes. The ashes had been cast into the current of the upper trade. A second example of the same kind occurred on January 20, 1835. On the 24th and 25th the sun was darkened in Jamaica by a shower of fine ashes, which had been discharged from the mountain Coseguina, distant 800 miles. The people learned in this way that the explosions previously heard were not those of artillery. These ashes could only have been carried by the upper current, as Jamaica lies north-east from the mountain. The same eruption gives also a beautiful proof that the ascending air-current divides itself above, for ashes fell upon the ship "Conway," in the Pacific, at a distance of 700 miles south-west of Coseguina.'

On a terrestrial globe I trace two meridians. At the equator of the globe they are a foot apart, which would correspond to about 1,000 miles on the earth's surface. But these meridians, as they proceed northward, gradually approach each other, and meet at the north pole. It is manifest that the air which rises between these meridians, in the equatorial regions, must, if it went direct to the pole, squeeze itself into an ever-narrowing bed. Were the earth a cylinder, instead of a sphere, we might have a circulation from the middle of the cylinder quite to each end, and a return current from

each end to the middle But this, in the case of the earth, is, as shown by Dove, impossible, simply because the space around the poles is unable to embrace the air from the equator. The cooled equatorial air sinks, and the return current sets in, before the poles are attained. The two currents, moreover, instead of flowing one over the other, often flow beside each other. They constitute rivers of air, with incessantly shifting beds.

These are the great winds of our atmosphere, which, however, are materially modified by the irregular distribution of land and water. Winds of minor importance also occur, through the local action of heat, cold, and evaporation. Such winds sometimes rush with sudden and destructive violence down the gulleys among mountains: gentler down-flows of gratefully cold air are produced by the presence of glaciers upon the heights. We have also land breezes and sea breezes, due to the varying temperature of the sea-board soil, by day and night. The morning sun, heating the land, produces vertical displacement, and the air from the sea moves landward. In the evening the land is more chilled superficially by radiation than the sea, and the conditions are reversed; the heavy air of the land now flows seaward.

MILDNESS OF THE EUROPEAN CLIMATE.

Thus, then, a portion of the heat of the tropics is sent, by an aërial messenger, towards the poles, a more equable distribution of terrestrial warmth being thus secured. But in its flight northward the air is accompanied by the vapour of water, which, you know, is perfectly transparent. Imagine the ocean of the tropics, giving forth its vapour, which promotes by its lightness the ascent of the associated air. Both expand as they ascend: at a height of 16,000 feet the air and

vapour occupy twice the volume which they embraced at the sea level. To secure this space they must, by their elastic force, push away the air in all directions round them; they perform work; and this work cannot be performed, save at the expense of the warmth with which they were, in the first instance, charged.

The vapour, thus chilled, is no longer competent to retain the gaseous form. It is precipitated, as cloud: the cloud descends as rain; and in the region of calms, or directly under the sun, where the air is first drained of its aqueous load, the descent of rain is enormous. The sun does not remain always vertically over the same parallel of latitude—he is sometimes north of the equator, sometimes south of it, the two tropics limiting his excursion. When he is south of the equator, the earth's surface, north of it, is no longer in the region of calms, but in one across which the aërial current from the north flows towards the region of calms. This moving air is but slightly charged with vapour, and, as it travels from north to south, it becomes ever warmer; it constitutes a dry wind, and its capacity to retain vapour is continually augmenting. It is plain, from these considerations, that each place between the tropics must have its dry season and rainy season; dry, when the sun is at the opposite side of the equator, and wet, when the sun is overhead.

Gradually, however, as the upper stream, which rises from the equator and flows towards the poles, becomes chilled and dense, it sinks towards the earth; at the Peak of Teneriffe it has already sunk below the summit of the mountain. With the contrary wind blowing at the base, the traveller often finds the wind from the equator blowing strongly over the top. Farther north the equatorial wind sinks lower still, and finally reaches the surface of the earth. Europe, for the most part, is overflowed

by this equatorial current. Here, in London, for eight or nine months in the year, south-westerly winds prevail. But mark what an influence this must have upon our climate. The moisture of the equatorial ocean comes to us endowed with potential energy; it comes, if you prefer the language, charged with latent heat. In our atmosphere condensation takes place, and the heat liberated is a main source of warmth to our climate. Were it not for the rotation of the earth, we should have over us the hot dry blasts of Africa; but, owing to this rotation, the wind which starts northward from the Gulf of Mexico is deflected to Europe. Europe is, therefore, the recipient of those stores of latent heat which were amassed in the western Atlantic. The British Isles come in for the greatest share of this moisture and heat, and this circumstance adds itself to that already dwelt upon—the high specific heat of water—to preserve our climate from extremes. It is this condition of things which makes our fields so green, and which also gives the freshness to our maidens' cheeks.

Another property of water, to which is probably due a large portion of its influence as a meteorological agent, shall be examined on a future occasion.

As we travel eastward in Europe, the amount of aqueous precipitation grows less and less; the air becomes more and more drained of its moisture. Even between the east and west coasts of our own islands, the difference is sensible; local circumstances, also, have a powerful influence on the amount of precipitation. Dr. Lloyd finds the mean yearly temperature of the western coast of Ireland to be about two degrees Fahr. higher than that of the eastern coast at the same elevation and in the same parallel of latitude. The total amount of rain which fell in the year 1851, at various stations in the island, is given in the following table:

Station	Rain in inches
Portarlington	21·2
Killough	23·2
Dublin	26·4
Athy	26·7
Donaghadee	27·9
Courtown	29·6
Kilrush	32·6
Armagh	33·1
Killybegs	33·2
Dunmore	33·5
Portrush	37·2
Burincrana	39·3
Markree	40·3
Castletownsend	42·5
Westport	45·9
Cahirciveen	59·4

With reference to this table, Dr. Lloyd remarks:

'1. That there is great diversity in the yearly amount of rain at the different stations, all of which (excepting four) are but a few feet above the sea level; the greatest rain (at Cahirciveen) being nearly three times as great as the least (at Portarlington).

'2. That the stations of least rain are either inland or on the eastern coast, while those of the greatest rains are at or near the western coast.

'3. That the amount of rain is greatly dependent on the proximity of a mountain chain or group, being always considerable in such neighbourhood, unless the station lie to the north-east of the same.

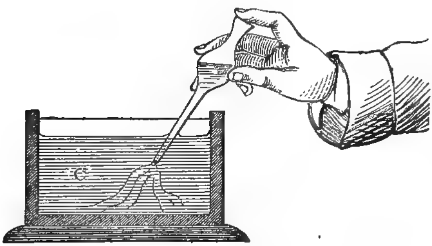
'Thus, Portarlington lies to the north-east of Slieve-bloom; Killough to the north-east of the Mourne range; Dublin, north-east of the Wicklow range, and so on. On the other hand, the stations of greatest rain, Cahirciveen, Castletownsend, Westport, &c., are in the vicinity of high mountains, but on a different side.' [1]

[1] The greatest rainfall recorded by Sir John Herschel in his table (Meteorology, p. 110, &c.) occurs at Cherra Pungee, where the annual fall is

CONVECTION IN LIQUIDS—THE GULF STREAM.

The distribution of heat by the transfer of heated air from place to place, is called, in England, '*convection*,' in contradistinction to the process of conduction, which will be treated in its proper place. Heat is distributed in a similar manner through liquids. This glass cell, c (fig. 65), contains warm water. Throwing, by means of a converging lens, a magnified image of the cell upon the

Fig. 65.



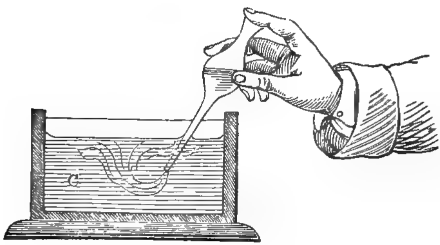
screen, I introduce the end of a pipette into the warm water of the cell, and allow a little cold water gently to enter it. The difference of refraction between them enables you to see the heavy cold water falling through the lighter warm water. The experiment succeeds still better when a fragment of ice is allowed to float upon the surface of the water. As the ice melts, it sends long heavy striæ downwards to the bottom of the cell.

We reverse the experiment by placing cold water in the cell, and hot water in the pipette. Care is here necessary to allow the warm water to enter without momentum, which would carry it mechanically down. The point of

592 inches. It is not my object to enter far into the subject of meteorology; for the fullest and most accurate information the reader will refer to the excellent works of Sir John Herschel, Mr. Buchan, Professor Loomis, and Professor Dove.

the pipette is now in the middle of the cell, and as the warm water enters, it speedily turns upwards (fig. 66) and spreads out at the top, almost as oil would do, under the same circumstances. In the Geyser of Iceland convec-

FIG. 66.



tion occurs on a grand scale. A fragment of paper thrown upon the centre of the water which fills the pipe, is instantly drawn towards the side, and there sucked down by the descending current.

Count Rumford made a number of very amusing but also very important experiments on the diffusion of heat through liquids. He had frequently noticed to his cost the tenacity with which stewed apples retained their heat. 'I never burnt my mouth with them,' he says, 'without endeavouring, but in vain, to find out some way of accounting for this most surprising phenomenon.' He noticed that the water of the volcanic bay of Baiæ was cold, while the sand on which the water lay was intolerably hot a few inches beneath the surface. Hence he concluded that water could not possess the power of conducting heat with which it was credited in his day. A sunbeam falling on a flask of heated alcohol which he had placed in a window to cool, revealed to him, by the motion of floating particles, the convection currents of the liquid. His final inference was that it is solely by such currents that liquids distribute their heat, and that if these

currents are impeded, a proportionate retardation of the diffusion occurs. The fibrous part of apples he found to amount to only two per cent. of the whole, the rest being mainly water. Still this small modicum of solid matter so reduced the power of transferring heat that while a thermometer surrounded by stewed apples required 535 seconds to be raised 80° F. in temperature, it required, when surrounded by water, only 172 seconds. Mixing 192 grains of starch with 2,276 grains of water, he found the convection so hampered by the starch that the heating of his thermometer 80° required 341 seconds of exposure, while when surrounded by pure water only 172 seconds were needed. The retention of heat by thick soup or chocolate is to be referred to the cause revealed by these experiments of Rumford.

An observation bearing on convection, which specially illustrates Rumford's penetration, was made by him on the Mer de Glace at Chamouni. He found there cylindrical shafts about 4 feet deep and 7 inches wide, filled with water which deepened in summer from day to day. How could this melting of the ice at the bottom occur? Remembering our experiments on the maximum density of water, Rumford's explanation is easily understood. The ice-cold water at the surface of the shaft, when warmed in the least degree by either air or sun, became denser, sank to the bottom of the shaft, melted its modicum of ice, and was continually displaced by the descent of fresh warmed water, the process of melting being thus maintained.

Partly by convection, and partly, perhaps, through the action of winds, currents establish themselves in the ocean, and powerfully influence climate, by the heat which they distribute. The most remarkable of these, and by far the most important for us, is the Gulf Stream, which crosses the Atlantic, from the equatorial regions, passing through the Gulf of Mexico, whence it derives its name.

As it quits the Straits of Florida it has a temperature of 83° Fahr.; thence it follows the coast of America as far as Cape Fear, whence it starts across the Atlantic, taking a north-easterly course, and finally washing the coast of Ireland, and the north-western shores of Europe generally.

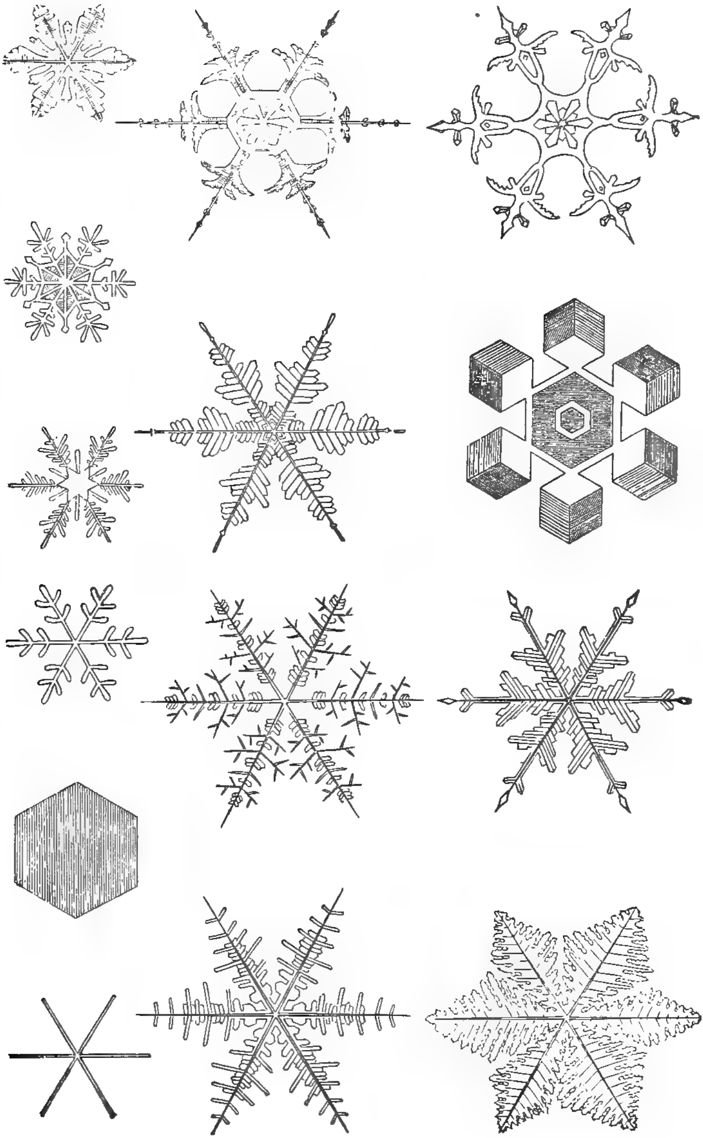
As might be expected, the influence of this body of warm water makes itself most evident during our winters. It then entirely abolishes the difference of temperature, due to the difference of latitude of north and south Britain; if we walk from the Channel to the Shetland Isles in January, we encounter everywhere the same temperature. The isothermal line runs north and south. The presence of this water renders the climate of Western Europe totally different from that of the opposite coast of America. The river Hudson, for example, in the latitude of Rome, is frozen for three months in the year. Starting from Boston in January, proceeding round St. John's, and thence to Iceland, we meet everywhere the same temperature. The harbour of Hammerfest derives great value from the fact that it is clear of ice all the year round. This is due to the Gulf Stream, which sweeps round the North Cape, and so modifies the climate there that at some places, on proceeding northward, you enter a warmer region. The contrast between Northern Europe and the east coast of America caused Halley to surmise that the north pole of the earth had shifted; that it was formerly situate somewhere near Behring's Straits, and that the intense cold, observed in these regions, is really the cold of the ancient pole, which had not been entirely subdued since the axis changed its direction. But the researches of the celebrated Dove have taught us that the Gulf Stream, and the diffusion of heat by winds and vapours, are the real causes of European mildness. On the western coast of America, between the Rocky Mountains and the ocean, we find a European climate.

Europe, then, is the condenser of the Atlantic; and the mountains are the chief condensers in Europe. On them, moreover, when they are sufficiently high, the condensed vapour descends, not in a liquid, but a solid form Let us look at this water in its birthplace, and follow it through its subsequent course. Clouds float in the air, and hence has arisen the surmise that they are composed of vesicles or bladders of water, thus forming *shells* instead of *spheres*. It is certain, however, as shown by Maxwell, that if the particles of water be sufficiently small, they will float for an indefinite period without being vesicular. It is also certain that water-particles at high elevations possess, on or after precipitation, the power of building themselves into crystalline forms; thus bringing forces into play which we have hitherto been accustomed to regard as molecular, and which could not be ascribed to the aggregates necessary to form vesicles.

SNOW.

Snow, perfectly formed, is not an irregular aggregate of ice-particles. In a calm atmosphere, the molecules arrange themselves, so as to form the most exquisite figures. You have seen those six-petalled flowers which show themselves within a block of ice, when a beam of heat is sent through it. The snow-crystals, formed in a calm atmosphere, are built upon the same type; the molecules arranging themselves to form hexagonal stars. From a central nucleus shoot six spiculæ, every two of which are separated by an angle of 60°. From these central ribs smaller spiculæ shoot right and left, with unerring fidelity to the angle 60°, and from these again other smaller ones diverge at the same angle. Dr. Scoresby has left us numerous drawings of the crystals of polar snow; those in fig. 67 have been copied from the sketches of Mr. Glaisher.

FIG. 67.



These frozen blossoms load the Alpine heights, where their frail architecture is soon destroyed by the weather. Every winter they fall, and every summer they disappear, but this rhythmic action does not perfectly compensate itself. Below a certain line, warmth is predominant, the quantity which falls every winter being entirely swept away; above this line, cold is predominant; the quantity which falls is in excess of the quantity melted, and an annual residue remains. In winter the snows stretch down to the plains; in summer they retreat to the *snow-line*, where the snow-fall of every year is exactly balanced by the consumption, and above which is the region of eternal snows. But, if a residue is left annually above the snow-line, the mountains must be loaded with a burden which increases every year. Supposing, at a particular point above the line referred to, a layer of three feet a year to be added annually to the mass; this deposit, accumulating even through the brief period of the Christian era, would produce an elevation of 5,630 feet. And did such accumulations continue throughout geologic, instead of historic ages, we cannot estimate the height to which the snows would pile themselves. It is manifest that no accumulation of this kind takes place. By some means or other the sun is prevented from lifting the ocean out of its basins, and piling its waters permanently upon the hills.

GLACIERS, REGELATION, MOULDING OF ICE BY PRESSURE.

How, then, is this annually augmenting load taken off the shoulders of the mountains? The snows sometimes detach themselves, and rush down the slopes in avalanches, melting to water in the warmer air below. But the violent rush of the avalanche is not their only motion; they also

creep, by almost insensible degrees, down the gentler slopes. As layer, moreover, heaps itself upon layer, the deeper portions of the mass become squeezed and consolidated; the air, first entrapped in the meshes of the snow, is forced out, and the compressed mass approximates more and more to the character of ice. You know how the granules of a snowball will adhere; and you know how hard you can make the ball if mischievously inclined. The snowball is incipient ice; augment the pressure, and you actually convert it into ice. But even after it has attained a compactness which would entitle it to be called ice, it is still capable of yielding more or less to pressure. When, therefore, a sufficient depth of the substance collects upon the earth's surface, the lower portions are squeezed out by the pressure of the upper ones, and if the snow rests upon a slope, it will yield principally in the direction of the slope, and move downwards.

This motion is incessantly going on along the slopes of every snow-laden mountain; in the Himalayas, in the Andes, in the Alps; but, in addition to this motion, which depends upon the power of the substance itself to yield to pressure, there is also a sliding motion, over the inclined bed. The consolidated snow moves bodily over the mountain slope, grinding off the asperities of the rocks, and polishing their hard surfaces. The under surface of the glacier is also scarred and furrowed by the rocks over which it has passed; but as the compacted snow descends, it enters a warmer region, is more copiously melted, and sometimes, before the base of its slope is reached, it is wholly cut off by fusion. Sometimes, however, large and deep valleys receive the gelid masses thus sent down; in these valleys it is further consolidated, and through them it moves, at a slow but measurable pace, imitating in all its motions those of a river. The ice is thus carried far beyond the limits of perpetual snow, until,

at length, the consumption below equals the supply above. At this point the glacier ceases. From the snow-line downwards in summer, we have *ice*; above the snow-line, both summer and winter, we have, on the surface, *snow*. The portion below the snow-line is called a *glacier*, that above the snow-line is called the *névé*, or *Firn*. The névé, or Firn, is the feeder of the glacier.

Several valleys, thus filled, may unite in a single valley, the tributary glaciers welding themselves together to form a common trunk. Both the main valley, and its tributaries, are often sinuous, and to form the trunk the tributaries must change their direction. The width of the valley, also, often changes: the glacier is forced through narrow gorges, widening after it has passed them; the centre of the glacier moves more quickly than the sides, and the surface more quickly than the bottom. The point of swiftest motion follows the same law as that observed in the flow of rivers, changing from one side of the centre to the other, as the flexure of the valley changes. Most of the great glaciers in the Alps have, in summer, a central velocity of two feet a day. There are points on the Mer-de-Glace, opposite the Montanvert, which have a daily motion of thirty inches in summer, and which, in winter, move at half this rate.

The physical property by which glacier ice is enabled to accommodate itself to the form of the Alpine channels through which it moves has been a subject of warm and prolonged discussion. Some writers have regarded the ice as viscous; some have referred its motion to its liquefaction under pressure (a point already illustrated in our Sixth Lecture), and the refreezing of the water in positions of diminished pressure; some have referred it to a quality observed by Faraday in 1850, in virtue of which ice is able to imitate in many respects the deportment of a viscous body. These are all true causes, and each of them pro-

bably comes more or less into play; but the cause which I have advocated as the principal one is that last mentioned. It is based upon the observed fact that when two pieces of moist ice, possessing throughout the temperature of 32° Fahr., are placed in contact with each other, they freeze together at the points where they touch.

To account for this freezing of a film of water, when the ice adjacent to it has no store of cold to be applied to freezing, has also given rise to considerable discussion. Some say that liquefaction, accompanied by refrigeration, occurs through pressure at the points of contact, and that the water formed, being below 32° in temperature, refreezes on escaping from the pressure, this new ice forming the cement which unites the contiguous surfaces. Others, under the leadership of Faraday, have reasoned in the following way. We know that vapour is continually escaping from the free surface of a liquid: that the particles at the surface attain their gaseous liberty sooner than the particles within the liquid; it is natural to expect a similar state of things with regard to ice—that when the temperature of a mass of ice is uniformly augmented, the first particles to attain liquid liberty will be those at the surface; for here they are entirely free, on one side, from the controlling action of the surrounding particles. Supposing, then, two pieces of ice, raised throughout to 32°, and melting, at this temperature, at their surfaces; what may be expected to occur if we place the liquefying surfaces close together? We thereby virtually transfer these surfaces to the centre of the ice, where the motion of each molecule is controlled, all around, by its neighbours. As might reasonably be expected, the liberty of liquidity, at each point where the surfaces touch each other, is arrested, and the two pieces freeze together at these points.

This is the effect to which attention was first directed by Faraday, in June 1850, and which is now known under

the name of *Regelation*.[1] On a hot summer's day, I have gone into a shop in the Strand, where fragments of ice were exposed in a basin in the window; and, with the shopman's permission, have laid hold of the topmost piece of ice, and, by means of it, have lifted the whole of the pieces bodily out of the dish. Though the thermometer at the time stood at 80°, the pieces of ice had frozen together at their points of junction. Even under hot water this effect takes place. The basin before me contains water as hot as my hand can bear; I plunge into it two pieces of ice and hold them together for a moment: they are now frozen to each other, notwithstanding the presence of the heated liquid. A pretty experiment of Faraday's consists in placing a number of small fragments of ice in a dish of water deep enough to float them. When one piece touches the other, even at a single point, regelation instantly sets in. Thus, a train of pieces may be caused to touch each other, and after they have once so touched, you may take the terminal piece of the train and, by means of it, draw all the others after it. When we seek to bend two pieces, thus united at their point of junction, the frozen points suddenly separate by fracture, but, at the same moment, other points come into contact, and regelation sets in between them. Thus a wheel of ice might be caused to roll on an ice surface, the contacts being incessantly ruptured, with a crackling noise, and others as quickly established by regelation. In virtue of this property of regelation, ice is able to reproduce many of the phenomena which are usually ascribed to viscous bodies.

Here, for example, is a straight bar of ice: by passing it successively through a series of moulds, each more curved than the last, it is finally turned out as a semi-

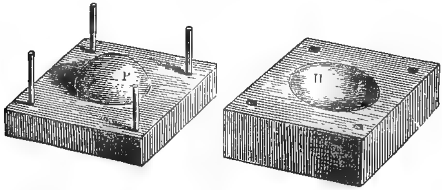
[1] A term suggested by Sir Joseph Hooker to Mr. Huxley and myself, on the publication of our first paper upon glaciers.

ring. The straight bar, on being squeezed into the curved mould, breaks; but by continuing the pressure, new surfaces come into contact, and the continuity of the mass is restored. A handful of small ice fragments, when squeezed together, freeze at their points of contact, and form one aggregate. The making of a snow-ball, as remarked by Faraday, illustrates the same principle. In order that this freezing shall take place, the snow ought to be at 32°, and moist. When below 32°, and dry, on being squeezed it behaves like salt. The crossing of snow-bridges, in the upper regions of the Swiss glaciers, is often rendered possible solely by the regelation of the snow granules. The climber treads down the mass carefully, and causes its granules to regelate: he thus obtains an amount of rigidity which, without the aid of regelation, would be quite unattainable. To those unaccustomed to such work, the crossing of snow bridges, spanning, as they often do, fissures 100 feet, and more, in depth, appears quite appalling.

Placing some ice fragments in a boxwood mould, of the form of a shallow cylinder, with a flat piece of boxwood overhead, I subject the ice to the action of a small hydraulic press, and squeeze the mass forcibly into the mould. The fragments are converted by the pressure into a coherent cake of ice. We can place it in a lenticular cavity and again squeeze it. It is crushed by the pressure, but new contacts are established, and the mass is turned into a lens of ice. I transfer the lens to a hemispherical cavity, H (fig. 68), bring down upon it a hemispherical protuberance, P, which is not quite able to fill the cavity, and squeeze the mass: the ice, which a moment ago was a lens, is now pressed into the space between the two spherical surfaces: on removing the protuberance, you see the interior surface of a cup of glassy ice. When detached from the mould, it is a hemispherical cup, which

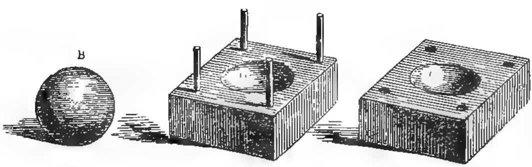
may be filled with cold wine, without the escape of a drop. I scrape, with a chisel, a quantity of ice from a

Fig. 68.



block, and, placing the spongy mass within a spherical cavity, C (fig. 69), squeeze it and add to it, till, finally, by bringing down upon it another spherical cavity, D, it is inclosed as a sphere between both. As the press is worked, the substance becomes more and more compact. I add more material, and again squeeze; by every such act the mass is made harder, and the result is a snowball such as you never saw before. It is a sphere of hard translucent ice, B. In this way broken ice can be rendered

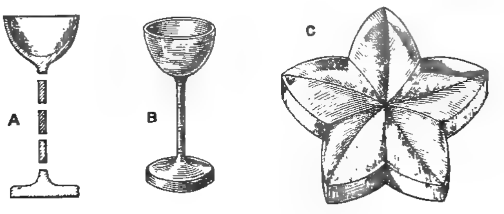
Fig. 69.



compact by pressure, and in virtue of the property of regelation, which cements its touching surfaces, the substance may be made to take any shape we please. Were the experiment worth the trouble, a rope of ice might be formed from this block, and afterwards coiled into a knot. Nothing, of course, can be easier than to produce statuettes of ice from suitable moulds. A (fig. 70) is made up of a cup, three short cylinders, and a foot, all of ice formed from squeezed snow. Put together, the pieces regelate

and form the claret glass B. C is also a coherent mass of ice produced by squeezing together smaller fragments. It is not difficult to understand, how a substance thus

FIG. 70.



endowed can be squeezed through the gorges of the Alps —can bend so as to accommodate itself to the flexures of the Alpine valleys, and can permit of a differential motion of its parts.[1]

ANCIENT GLACIERS, THEORETIC ERRORS.

I have thus briefly sketched the phenomena of existing glaciers, as far as they are related to our present subject; but the scientific explorer of mountain regions soon meets with appearances, which carry his mind back to a state of things very different from that of the present day. The unmistakable traces which they have left behind them show that vast glaciers once existed, in places from which they have for ages disappeared. This was first proved by Venetz, an engineer of Brieg in the valley of the Rhone. Go, for example, to the glacier of the Aar in the Bernese Alps, and observe its present performances; look to the rocks upon its flanks as they are at this moment, rounded, polished, and scarred by the moving ice. And, having by patient and

[1] In moulding ice, it is advisable to first wet the boxwood mould with hot water. This facilitates the removal of the compressed substance. A conical plug is inserted into my own moulds, the careful tapping of which soon detaches the ice.

varied exercise educated your eye and judgment in these matters, walk down the glacier towards its end, keeping always in view the evidences of glacial action. After quitting the ice, continue your walk down the valley towards the Grimsel: you see everywhere the same unmistakable record. The rocks which rise from the bed of the valley are rounded like hogs' backs. These are the *roches moutonnés* of Charpentier and Agassiz; you observe upon them the larger flutings of the ice, and also the smaller scars, scratched by pebbles, which the glacier held as a kind of emery on its under surface. All the rocks of the Grimsel have been thus planed down. Walk down the valley of Hasli and examine the mountain sides right and left; without the key, which I now suppose you to possess, you would be in a land of enigmas; but with this key all is plain—you see everywhere the well-known scars and flutings and furrowings. In the bottom of the valley you have the rocks rubbed down, in some places, to dome-shaped masses, and, in others, polished so smoothly that to pass over them, even when the inclination is moderate, steps must be hewn. All the way down to Meyringen, and beyond it, if you wish to pursue the inquiry, these evidences abound. For a preliminary lesson in recognising the traces of ancient glaciers, no better ground than this can be chosen.

Similar evidences are found in the valley of the Rhone; you may track them through the valley for eighty miles, and lose them at length in the Lake of Geneva. But on the flanks of the Jura, at the opposite side of the Canton de Vaud, the evidences reappear. All along these limestone slopes are strewn the granite boulders of Mont Blanc. Right and left, also, from the great Rhone valley, the lateral valleys show that they were once filled with ice. On the Italian side of the Alps the remains are still more stupendous than those on the northern side,

Grand as the present glaciers seem to those who explore them to their full extent, they are mere pigmies in comparison with their predecessors.

Not in Switzerland only—not only in proximity with existing glaciers—are these well-known vestiges of the ancient ice discernible; on the hills of Cumberland they are almost as clear as among the Alps. Where the bare rock has been exposed for ages to the action of the weather, the finer marks have, in many cases, disappeared; and the mammillated forms of the rocks are the only evidences. But the removal of the protecting soil often discloses surfaces, scarred as sharply, and polished as cleanly, as those which are now being scratched and polished by the glaciers of the Alps. Round about Scawfell, the traces of ancient ice appear, both in *roches moutonnés* and *blocs perchés*; and there are ample facts to show that Borrodale was once occupied by glacier ice. On North Wales, also, the ancient glaciers have placed their stamp so firmly, that the ages which have since elapsed have failed to obliterate even their superficial markings. All round Snowdon these evidences abound. On the south-west coast of Ireland rise the Reeks of Macgillicuddy, which tilt upwards, and catch upon their cold crests the moist winds of the Atlantic; precipitation is copious, and rain at Killarney seems the order of Nature. In this moist region every crag is covered with rich vegetation; but the vapours, which now descend as mild and fertilising rain, fell aforetime as snow, which formed the material for noble glaciers. The Black Valley was once filled by ice, which planed down the sides of the Purple Mountain, as it moved towards the Upper Lake. The basin occupied by this lake was entirely filled by the ancient ice, and every island that now emerges from its surface is a glacier-dome. The fantastic names, which many of the

rocks have received, are suggested by the shapes into which they have been sculptured by the mighty moulding plane which once passed over them. North America is also thus glaciated. A notable observation, in connection with this subject, has been made by Sir Joseph Hooker during a visit to Syria. He found the celebrated cedars of Lebanon growing upon ancient glacier moraines.

To determine the conditions which permitted of the formation of those vast masses of ice, has long been a problem with philosophers, and a consideration of some of the solutions which have been offered, from time to time, will not be uninstructive. The aim of all the writers on this subject, with whom I was acquainted when I took it up, was the attainment of *cold*. Some eminent men thought that the reduction of temperature, during the glacial epoch, was due to a diminution of solar radiation; others thought that in its motion through space our system may have traversed regions of low temperature, and that, during its passage through these regions, the ancient glaciers were produced. Others tried to lower the temperature, by a redistribution of land and water. The eminent men who propounded and advocated the above hypotheses, appear, one and all, to have overlooked the fact, that the enormous extension of glaciers in bygone ages demonstrates, just as rigidly, the operation of heat as the action of cold.

Cold alone will not produce glaciers. You may have the bitterest north-east winds here in London throughout the winter, without a single flake of snow. Cold must have the fitting object to operate upon, and this object—aqueous vapour—is the direct product of heat. Let us put this glacier question in another form: the latent heat of aqueous vapour, at the temperature of its production in the tropics, is about 1,000° Fahrenheit.

A pound of water, then, vaporised at the equator, has absorbed 1,000 times the quantity of heat which would raise a pound of the liquid one degree in temperature. But the quantity of heat which would raise a pound of water one degree, would raise a pound of cast iron ten degrees: hence, simply to convert a pound of the water of the equatorial ocean into vapour, a quantity of heat would be required sufficient to impart to a pound of cast iron 10,000 degrees of temperature. But the fusing-point of cast iron is 2,000° Fahr.; therefore, for every pound of vapour produced, a quantity of heat has been expended by the sun, sufficient to raise 5 lbs. of cast iron to its melting point. Imagine, then, every one of those ancient glaciers with its mass quintupled; and imagine, in the place of the mass so augmented, an equal weight of cast iron raised to the white heat of fusion; we shall then have the exact expression of the solar action involved in the production of the ancient glaciers. Substitute the hot iron for the cold ice—our speculations would instantly be directed to account for the *heat* of the glacial epoch instead of its cold, and a complete reversal of some of the hypotheses above quoted would ensue.

It is perfectly manifest, that by weakening the sun's action, either through a defect of emission, or by the steeping of the entire solar system in space of a low temperature, we should be cutting off the glaciers at their source. Vast masses of mountain ice indicate, infallibly, the existence of commensurate masses of atmospheric vapour, and a proportionately vast action on the part of the sun. In a distilling apparatus, if you required to augment the quantity distilled, you would surely not attempt to obtain the low temperature necessary to condensation, by taking the fire from under your boiler; but this is what has been done by those philosophers who have sought to produce the ancient glaciers by diminishing the sun's heat. It is quite manifest that the thing most

needed to produce the glaciers is an *improved condenser*; we cannot afford to lose an iota of solar action; we need, if anything, more vapour, but we need a condenser so powerful, that this vapour, instead of falling in liquid showers to the earth, shall be so far reduced in temperature as to descend in snow. The problem, I think, is thus narrowed to the precise issue on which its solution depends.

LECTURE IX.

CONDUCTION OF HEAT—GOOD CONDUCTORS AND BAD CONDUCTORS—CONDUCTIVITY OF THE METALS FOR HEAT: RELATION BETWEEN THERMAL AND ELECTRIC CONDUCTIVITY—INFLUENCE OF TEMPERATURE ON THE CONDUCTION OF ELECTRICITY—INFLUENCE OF MOLECULAR CONSTITUTION ON THE CONDUCTION OF HEAT—RELATION OF SPECIFIC HEAT TO CONDUCTION—PHILOSOPHY OF CLOTHES: RUMFORD'S EXPERIMENTS—INFLUENCE OF MECHANICAL TEXTURE ON CONDUCTION—INCRUSTATIONS OF BOILERS—THE SAFETY LAMP—CONDUCTIVITY OF LIQUIDS AND GASES.

CONDUCTION OF HEAT—EXPERIMENTAL ILLUSTRATIONS.

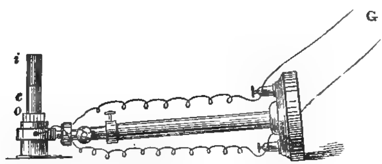
THE conception of molecular motion is now, I trust, familiar to us; and we have to-day to carry this conception a step forward. Closely grouped as they are, the atoms of solid bodies cannot oscillate without communicating their motion to neighbouring atoms. To this propagation of the motion of heat from atom to atom, we must now devote our attention.

I grasp this poker, and feel it to be a hard and heavy body, but it neither warms nor chills me. It has been before the fire, and its temperature, at the present moment, chances to be the same as that of my nerves; there is neither abstraction nor communication of heat. But when the end of the poker is thrust into the fire, its atoms are thrown into a state of more intense oscillation; these swinging atoms strike their neighbours, these again theirs, and thus the molecular music rings along the bar. If I were now to lay hold of the

poker, its atomic motion would be communicated to my nerves, and would produce the pain of burning. Convection we have already defined to be the transfer of heat, by sensible masses of matter, from place to place; but the transfer which consists in each *atom* taking up the motion of its neighbours, and sending it on to others, is called the *conduction* of heat.

Let me exemplify this property of conduction, in a homely way. In a basin, filled with warm water, I have placed a cylinder of iron, an inch in diameter, and two inches in height. This cylinder is to be my source of heat. Laying the thermo-pile, *o* (fig. 71), thus flat, with its naked

FIG. 71.



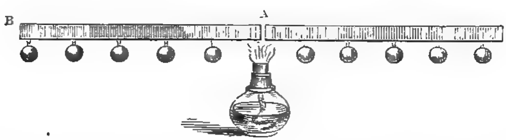
face turned upwards, I set upright upon that face a cylinder of copper, *c*, which now possesses the temperature of this room. You observe no deflection of the galvanometer. I now place the warm cylinder, *i*, having first dried it, upon the cool one. The upper cylinder is not at more than blood-heat; but you see, almost before this remark is uttered, the needle flies aside. The heat of the iron cylinder has been thus rapidly 'conducted' by the copper one to the face of the pile.

Copper, which we have just used, possesses this power of conduction in a very eminent degree. Let us now remove the copper, allow the needle to return to 0°, and then lay upon the face of the pile a cylinder of glass. On the cylinder of glass I place, as before, the iron cylinder, which has been re-heated in the warm water. We wait thrice the time required by the copper to

transmit the heat, but the needle continues motionless. Placing cylinders of wood, chalk, stone, and fire-clay, in succession, on the pile, and heating their upper ends in the same manner, we find that in the time which we can devote to an experiment, not one of these substances is competent to transmit the heat to the pile. The molecules of these substances are incompetent to pass the motion of heat freely from one to another. These bodies are all *bad conductors* of heat. On the other hand, when cylinders of zinc, iron, lead, bismuth, &c., are placed in succession on the pile, each of them exhibits the power of transmitting the motion of heat rapidly through its mass. In comparison with the wood, stone, chalk, glass, and clay, they are all *good conductors* of heat.

As a general rule, not however without exception, metals are the best conductors of heat. But metals differ notably among themselves, as regards their powers of conduction. A comparison of copper and iron will illustrate this point. Behind me are two bars, A B, A C (fig. 72),

FIG. 72.



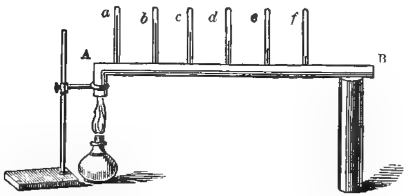
placed end to end, with balls of wood, attached by wax at equal distances from the place of junction. Under the junction is placed a spirit-lamp, which heats the ends of the bars: the heat is propagated right and left through both. The bar A B is iron, the bar A C is copper; the heat travels to a greater distance along the copper, which is the better conductor, this being proved by the liberation of a greater number of its balls.

One of the first attempts to determine, with accuracy, the conductivity of different bodies for heat, was that

suggested by Franklin, and carried out by Ingenhausz. He coated a number of bars of various substances with wax, and, immersing the ends of the bars in hot oil, he observed the distance to which the wax was melted, on each of the bars. The good conductors melted the wax to the greatest distance; and the melting distance furnished a measure of the conductivity of the bar.

The second method was that indicated by Fourier, and followed out experimentally by Despretz. A B (fig. 73)

FIG. 73.



represents a bar of metal, with holes drilled in it, intended to contain small thermometers. At the end of the bar was placed a lamp, as a source of heat; the heat was propagated through the bar, reaching the thermometer *a* first, *b* next, *c* next, and so on. For a certain time, the thermometers continued to rise, but the state of the bar became at length stationary, each thermometer marking a constant temperature. The better the conductor, the smaller was the difference between any two successive thermometers. The decrement, or *fall* of heat, if I may use the term, from the hot end towards the cold, is greater in bad conductors than in good ones, and, from the decrement of temperature shown by the thermometers, we can deduce, and express by a number, the conductivity of the bar. This method was also followed by MM. Wiedemann and Franz, in a very important investigation, but, instead of using thermometers, they employed a suitable modification of the thermo-pile. Of the numerous and highly

interesting results of this investigation, the following is a tabular résumé:

Name of Substance	Conductivity For Heat	Conductivity For Electricity
Silver	100	100
Copper	74	73
Gold	53	59
Brass	24	22
Tin	15	23
Iron	12	13
Lead	9	11
Platinum	8	10
German Silver	6	6
Bismuth	2	2

This table shows, that, as regards their powers of conducting heat, metals differ very widely from each other. Calling, for example, the conductive power of silver 100, that of German silver is only 6. You may illustrate this difference, in a very simple way, by plunging two spoons, one of German silver, and the other of pure silver, into the same vessel of hot water. After a little time, you find the free end of the silver spoon much hotter than that of its neighbour; and if bits of phosphorus be placed on the ends of the spoons, that on the silver will fuse and burn, while the heat transmitted through the other spoon never reaches an intensity sufficient to ignite the phosphorus.

RELATION OF THERMAL TO ELECTRICAL CONDUCTION.

To discern connections and relations between the powers of nature gratifies the causal and unifying tendency of the human mind. We know that they are interdependent, we know that they are mutually convertible, but, as yet, we know very little as to the precise form of the conversion. We have, for example, every reason to conclude, that heat and electricity are both modes of motion; we know, experimentally, that from electricity we can obtain heat, while from heat, as in the case of our thermo-

pile, we can obtain electricity. But although we have, or think we have, tolerably clear ideas of the character of the motion of heat, our ideas as to the precise nature of the change which this motion must undergo in order to appear as electricity are still very defective.

The above table is a spur to further investigation. Beside the numbers expressing conductivity for heat, MM. Wiedemann and Franz have placed the numbers expressing the conductivity of the same metals for electricity. They run side by side: the good conductor of heat is the good conductor of electricity, and the bad conductor of heat is the bad conductor of electricity.[1] Thus, we may infer that the same physical quality which interferes with the transmission of heat, interferes, in a proportionate degree, with the transmission of electricity. This common susceptibility of both agents indicates a relation, on which future investigations will no doubt throw light.

It is a proved fact, that the amount of heat developed in a wire, by a current of electricity of a certain strength, is directly proportional to the resistance of the wire.[2] Holding on to the conception of a current, we may in the case of a bad conductor imagine the arrangement of the atoms to be such as to set up a kind of friction between them and the current, which is thus enabled to impart its motion to the atoms, and to render the wire hot. In the case of a good conductor the current may be pictured as gliding freely among the atoms, without disturbing them in any considerable degree. Suspended before you are some pieces of platinum wire, each four or five inches long, joined alternately to pieces of silver wire, of the same length and thickness. Sending from a battery of forty of Grove's cells the self-same current through

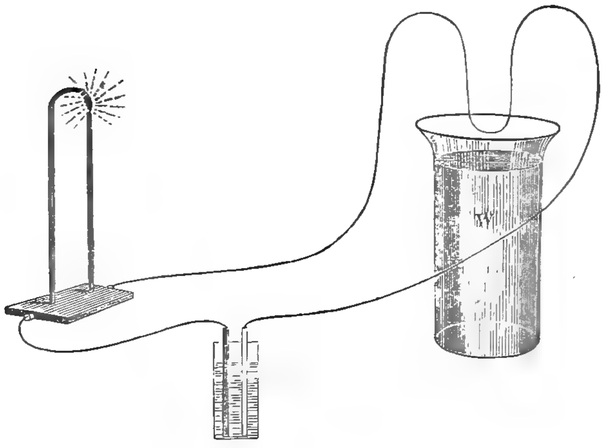
[1] Principal Forbes had previously noticed this. See Phil. Mag. 1834, vol. iv. p. 27.

[2] Joule, Phil. Mag. 1841, vol. xix. p. 263.

this compound wire, some spaces appear white-hot, with dark spaces between them. The white-hot portions of the compound wire are platinum, and the dark portions are silver. The electric current breaks, as it were, impetuously upon the atoms of the platinum, while it glides, with little resistance, among those of silver, thus producing, in the two metals, different calorific effects.

The motion of heat, moreover, interferes with that of electricity. The little lamp, which stands in front of the

FIG. 74.



table, consists, as you know, of a coil of platinum wire, suitably attached to a brass stand. On sending a current through that coil we cause it to glow. Into the circuit are also introduced two additional feet of thin platinum wire, and, on establishing the connection, both it and the lamp are raised to vivid redness. What I wish now to prove is, that the heat, which the electricity has generated in these two feet of wire, offers a hindrance to the passage of the current, which has thus raised up a foe in its own path. If we cool this wire, we shall open a wider door for

the passage of the electricity. But, if more electricity passes, it will announce itself at the platinum lamp, raising the red heat to whiteness, the change in the intensity of the light being visible to you all.

Thus, then, I plunge the red-hot wire into a beaker of water W (fig. 74): the lamp immediately becomes almost too bright to look at. When the wire is raised out of the water, and the heat is allowed once more to develop itself, the current is instantly impeded, and the lamp becomes less bright. I again dip the wire into the cold water, but this time deep enough to quench the whole of it. The augmented current raises the lamp to its maximum brightness, and puts it suddenly out. The circuit, in fact, is now broken, for the platinum coil has actually been fused by the additional flow of electricity.

To all appearance, cold may be conducted as well as heat. I warm a copper cylinder by holding it, for a moment, in my hand. When placed on the pile, the needle goes up to 90°, declaring heat. On this warm cylinder, I place a second one, which has been chilled, by sinking it for some time in ice. We wait a moment, the needle moves: it is now descending to zero, passes it, and goes to 90°, on the side of cold. Analogy might lead you to suppose that the cold is conducted downwards, from the top cylinder to the bottom one, like the heat in our former experiments. No objection need be made to the phrase 'conduction of cold,' if it be used with a clear knowledge of the physical process involved. In the case here before us the warm intermediate cylinder first delivers up its heat, or motion, to the cold cylinder overhead, and, having thus lost its own heat, it draws upon that of the pile. In our former experiments, we had conduction of motion *to* the pile; in our present one, we have conduction of motion *from* the pile. But it is,

in both cases, the propagation of motion with which we have to deal, the heating and the chilling depending solely upon the *direction* of propagation. I place one of these metal cylinders, which has been purposely cooled, on the face of our pile; a violent deflection follows, declaring the instrument to be chilled. Are we to suppose cold to be an entity communicated to the pile? No. The pile here is the warm body; its molecular motion is in excess of that possessed by the cylinder; and when both come into contact, the pile imparts a quantity of its motion to the cylinder, and, by its own bounty, becomes impoverished: it chills itself, and generates the current.

Substituting for the cold metal cylinder a wooden cylinder of the same temperature, the chill is very feeble, and the consequent deflection very small. Why does not the cold wood produce an action equal to that of the cold metal? Simply, because the heat communicated to it by the pile is accumulated at its under surface; it cannot escape through the bad conducting wood as it escapes through the metal, and thus the quantity of heat withdrawn from the pile by the wood, is less than that withdrawn by the metal. A similar effect is produced when the human nerves are substituted for the pile. When you come into a cold room, and lay your hand upon the fire-irons, the chimney-piece, the chairs, and the carpet, in succession, they appear to be of different temperatures; the iron chills you more than the marble, the marble more than the wood, and so on. Your hand is affected exactly as the pile was affected in the last experiment. It is needless to say that the reverse takes place when you enter a hot room. You would certainly suffer, if you lay down upon a plate of metal in a Turkish bath; but you do not suffer when you lie down on a bench of wood. By preserving the body from contact with good conductors, very high temperatures may

be endured. Eggs may be boiled, and beefsteaks cooked, by the heat of an apartment, in which the bodies of living men sustain no injury.

The philosophy of this last experiment is worthy of a moment's consideration. With it the names of Blagden and Chantrey are associated, those eminent men having exposed themselves in ovens to temperatures considerably higher than that of boiling water. Let us compare the condition of the two living human beings with that of two marble statues, placed in the same oven. The statues become gradually hotter, until finally they assume the temperature of the air of the oven; the two men, under the same circumstances, do not similarly rise in temperature. If they did, the tissues of the body would be infallibly destroyed, the temperature endured being more than sufficient to stew the muscles in their own liquids. Here the excess of heat, instead of being applied to increase the temperature of the body, is applied to change its aggregation; the heat prepares the perspiration, forces it through the pores, and vaporises it. Heat is thus consumed in work. This is the waste-pipe, if I may use the term, through which the excess overflows. Some people have professed to see, in this power of the living body to resist a high temperature, a conservative action, peculiar to the vital force. No doubt, all the actions of the animal organism are connected with what we call its vitality; but the action here referred to is the same in kind as the melting of ice, or the vaporisation of water. It consists simply in the diversion of heat from the purposes of temperature to the performance of work.

INFLUENCE OF MOLECULAR STRUCTURE.

Thus far, we have compared the conducting power of different bodies together; but the same substance may

possess different powers of conduction in different directions. Many crystals are so built, that the motion of heat runs with greater facility along certain lines than along others. That heat travels with greater facility along the axis of rock-crystal than across the axis has been proved in a very simple manner by M. de Senarmont. Of these two plates of quartz, one (fig. 76) is cut perpendicularly to the

FIG. 75.

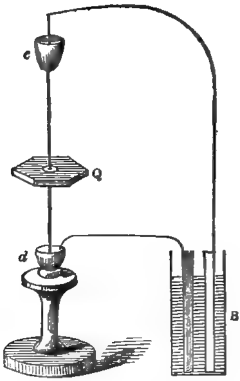


FIG. 76.

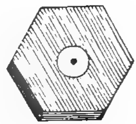
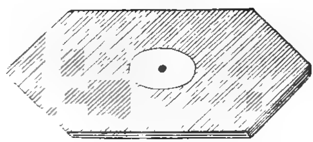


FIG. 77.



axis of the crystal, and the other (fig. 77) parallel to it. The plates are coated with a layer of white wax, laid on by a camel's-hair pencil. They are pierced at the centre, and into the hole is inserted a small sewing-needle, which can be warmed by an electric current. B (fig. 75) is the battery, whence the current proceeds; *c* is a capsule of wood, through the bottom of which the sewing-needle passes; *d* is a second capsule, into which dips the point of the needle, and Q is the perforated plate of quartz. Each capsule contains a drop of mercury. When the current passes from *c* to *d*, the needle is heated, and the heat is propagated in all directions. The wax melts around the place where the heat is applied, and on the plate which is cut perpendicularly to the axis of the quartz,

the figure of the melted wax is a perfect circle (fig. 76). In the other plate the heat travels more readily along the axis than across it, and hence the wax figure is an ellipse, instead of a circle (fig. 77). Iceland spar also conducts better along the crystallographic axis than at right angles to it, while a crystal of tourmaline conducts best at right angles to its axis. The metal bismuth, with which you are already acquainted, cleaves with great facility in one direction, and, as was shown by Svanberg and Matteucci, it conducts both heat and electricity better along the planes of cleavage than across them.

CONDUCTION OF HEAT BY WOOD, ETC.

In wood, we have an eminent example of this difference of conductivity. Many years ago MM. de la Rive and De Candolle instituted an inquiry into the conductive power of wood,[1] and, in the case of five specimens examined, established the fact that the velocity of transmission was greater along the fibre than across it. The manner of experiment was that usually adopted in inquiries of this nature, and which was applied to metals by Despretz.[2] The end of a bar of the substance was brought into contact with a source of heat, and allowed to remain there until a state of equilibrium was assumed. The temperatures attained by the bar, at various distances from its heated end, were ascertained by means of thermometers fitting into cavities made to receive them; from these data, with the aid of a well-known formula, the conductivity of the wood was determined.

To determine the velocity of calorific transmission, in different directions, through wood, the instrument shown in fig. 78 was devised, some years ago, by myself. Q Q′, R R′

[1] Mém. de la Soc. de Genève, vol. iv. p. 70.

[2] Annales de Chim. et de Phys., December 1827.

Fig. 78.

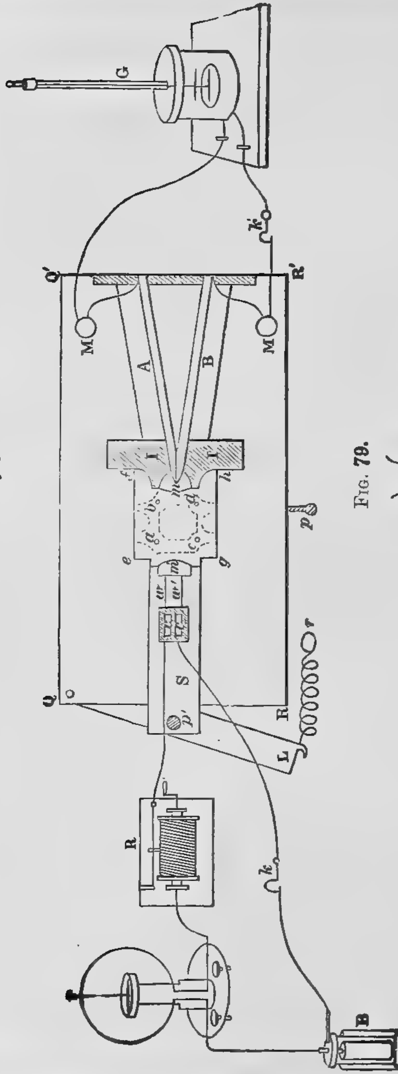


Fig. 79.

is an oblong piece of mahogany, A is a bar of antimony, B is a bar of bismuth. The touching ends of the two bars are kept in close contact by the ivory jaws I I′, the other ends being let into a second piece of ivory, in which they are firmly fixed. From these ends proceed two pieces of platinum wire to the little ivory cups M M, communicating with drops of mercury placed in the cups. Two small projections are observed in the figure, jutting from I I′; from one projection to the other, a fine membrane is stretched, thus inclosing a little chamber *m*, in front of the wedge-like end of the bismuth and antimony pair; the chamber has an ivory bottom. S is a wooden slider, which, by means of the lever L, can be moved smoothly backward and forward along a bevelled groove. This lever turns on a pivot at Q, and fits into a horizontal slit in the slider, to which it is attached by the pin *p*′ passing through both. In the lever an oblong aperture is cut, through which *p*′ passes, and in which it has a certain amount of lateral play, so as to enable it to push the slider forward in a straight line. Two projections are seen at the end of the slider, across which a thin membrane is stretched; a chamber *m*′ is thus formed, with three sides and a floor of wood, and bounded in front by the membrane. A thin platinum wire, bent up and down several times, so as to form a kind of grating, is laid against the back of the chamber *m*′, and imbedded in the end of the slider by the stroke of a hammer. The end is filed down, until about half the wire is removed, and the whole reduced to a uniform flat surface. Against the common surface of the slider and wire, an extremely thin plate of mica is glued, sufficient, simply, to interrupt all contact between the bent wire and a drop of mercury, which the chamber *m*′ is destined to contain; the ends *w w*′ of the bent wire proceed to two small cisterns *c c*′, hollowed out in a slab of ivory, and filled with

mercury. The end of the slider and its bent wire are shown in fig. 79. The rectangular space *e f g h* (fig. 78) is cut quite through the slab of mahogany, and a brass plate is screwed to the latter underneath; from this plate (which is cut away, as shown by the dotted lines in the figure) four conical ivory pillars *a b c d* project upwards. Though appearing to be upon the same plane as the upper surfaces of the bismuth and antimony bars, the points of the pillars are in reality 0·3 of an inch below the said surfaces.

The small cube to be examined is placed, by means of a pair of pliers, upon the four supports *a b c d*; the slider S is then drawn up against the cube, which is firmly clasped between the projections of the piece of ivory I I′ on the one side, and those of the slider S on the other. The chambers *m* and *m′* being filled with mercury, the membrane in front of each is pressed gently against the cube by the interior fluid mass, and, in this way, a uniform contact, which is absolutely essential, is secured.

The problem now is to apply a source of heat, of a strictly measurable character, and always readily attainable, to the face of the cube in contact with the membrane *m′* at the end of the slider, and to determine what quantity of this heat crosses the cube to the opposite face, during a minute of time.

To obtain a source of heat the following method was adopted: B is a small galvanic battery, from which a current proceeds to the tangent compass T; passes round the ring of the instrument, deflecting in its passage the magnetic needle, which hangs in the centre of the ring. From T the current proceeds to the rheostat R. This instrument consists of a cylinder of serpentine stone, round which a German-silver wire is coiled spirally; by turning the handle of the instrument, any required quantity of this powerfully resisting wire is thrown into the circuit,

the current being thus regulated at pleasure. The sole use of these last two instruments, in the present series of experiments, is to keep the current constant from day to day. From the rheostat the current proceeds to the cistern c, thence through the bent wire, and back to the cistern c', from which it proceeds to the other pole of the battery.

The bent wire, during the passage of the current, is gently heated; the heat is transmitted through the mercury in the chamber m' to the membrane in front of the chamber; this membrane becomes the proximate source of heat applied to the cube. The quantity of heat transmitted from this source, through the mass of the cube, to the opposite face, in any given time, is estimated from the deflection which it is able to produce upon the needle of a galvanometer, connected with the bismuth and antimony pair. G is the galvanometer, from which wires proceed to the mercury cups M M.

The action of mercury upon bismuth, as a solvent, is well known; an amalgam is speedily formed when the two metals come into contact. To preserve the thermo-electric couple from this action, their ends are protected by a sheathing of the same membrane as that used in front of the chambers m m'.

Pressed by the mercury, the two membranes in front of m and m' bulge out a little, thus forming a pair of soft and slightly convex cushions. When the cube is placed on its supports, and the slider is brought up against it, both cushions are pressed flat, and thus the contact is made perfect. The cube is always firmly caught between the opposed rigid projections, the slider being held fast in this position by means of the spring r, which is then attached to the pin p. The mode of experiment is this: Having first seen that the needle of the galvanometer points to zero when the

thermo-circuit is complete, the latter is interrupted by means of the break-circuit key k'. At a certain moment, marked by the second hand of a watch, the voltaic circuit is closed by the key k, and the current is permitted to circulate for sixty seconds; at the sixtieth second the voltaic circuit is broken, by the left hand at k, while, at the same instant, the thermo-electric circuit is closed by the right hand at k'. The needle of the galvanometer is instantly deflected, and the limit of the first impulsion is noted. The amount of this impulsion depends, of course, upon the quantity of heat which has reached the bismuth and antimony junction, through the mass of the cube, during the time of action. The cube being removed the instrument is allowed to cool, until the needle of the galvanometer returns again to zero.

Judging from the description, the mode of working may appear complicated, but, in reality, it is not so. A single experimenter has the most complete command over the entire arrangement. The wires from the small galvanic battery (a single cell) remain undisturbed from day to day; all that is to be done is to connect the battery with them, and everything is ready for experiment.

There are in wood three lines, at right angles to each other, which the mere inspection of the substance enables us to fix upon, as the necessary resultants of molecular action: the first line is parallel to the fibre; the second is perpendicular to it, and to the ligneous layers which indicate the annual growth of the tree; while the third is perpendicular to the fibre, and parallel, or rather tangential, to the layers. From each of a number of trees a cube was cut, two of the faces being parallel to the ligneous layers, two perpendicular to them, while the remaining two were perpendicular to the fibre. It was

proposed to examine the velocity of calorific transmission through the wood, in these three directions. It may be remarked, that the wood was in all cases well-seasoned and dry.

The cube, each edge of which was 0·3 of an inch, was first placed upon its four supports *a b c d*, so that the line of flux from m' to m was parallel to the fibre, and the deflection produced by the heat transmitted in sixty seconds, was observed. The cube was then placed with its fibre vertical, the line of flux from m' to m being perpendicular to the fibre, and parallel to the ligneous layers; the deflection produced by a minute's action, in this case, was also determined. Finally, the cube was turned 90° round, its fibre being still vertical, so that the line of flux was perpendicular to both fibre and layers, and the consequent deflection was observed. In the comparison of these two latter directions, the chief delicacy of manipulation is necessary. It requires but a rough experiment, to demonstrate the superior velocity of propagation along the fibre; but the velocities in all directions perpendicular to the fibre are so nearly equal, that it is only by great care, and, in the majority of cases by numerous experiments, that a difference of action can be securely established.

The following table contains some of the results of the inquiry: it will explain itself:

Description of wood	Deflections I. Parallel to fibre	Deflections II. Perpendicular to fibre and parallel to ligneous layers	Deflections III. Perpendicular to fibre and to ligneous layers
	°	°	°
1 American birch . .	35	9·0	11·0
2 Oak	34	9·5	11·0
3 Beech	33	8·8	10·8
4 Coromandel-wood . .	33	9·8	12·3
5 Bird's-eye maple . .	31	11·0	12·0
6 Lance-wood . . .	31	10·6	12·1
7 Box-wood . . .	31	9·9	12·0
8 Teak-wood . . .	31	9·9	12·4
9 Rose-wood . . .	31	10·4	12·6
10 Peruvian-wood . .	30	10·7	11·7
11 Green-heart . . .	29	11·4	12·6
12 Walnut . . .	28	11·0	13·0
13 Drooping ash . .	28	11·0	12·0
14 Cocoa-wood . . .	28	11·9	13·6
15 Sandal-wood . . .	28	10·0	11·7
16 Tulip-wood . . .	28	11·0	12·1
17 Camphor-wood . .	28	8·6	10·0
18 Olive-tree . . .	28	10·5	13·2
19 Ash	27	9·5	11·5
20 Black oak . . .	27	8·0	9·4
21 Apple-tree . . .	26	10·0	12·5
22 Iron-wood . . .	26	10·2	12·4
23 Chestnut . . .	26	10·1	11·5
24 Sycamore . . .	26	10·6	12·2
25 Honduras mahogany .	25	9·0	10·0
26 Brazil-wood . . .	25	11·9	13·9
27 Yew	24	11·0	12·0
28 Elm	24	10·0	11·5
29 Plane-tree . . .	24	10·0	12·0
30 Portugal laurel . .	24	10·0	11·5
31 Spanish mahogany .	23	11·5	12·5
32 Scotch fir . . .	22	10·0	12·0

The results of De la Rive and De Candolle, regarding the superior conductivity of the wood in the direction of the fibre, are here corroborated. Evidence is also afforded, as to how little mere density affects the velocity of transmission. There appears to be neither law nor general rule here. American birch, a comparatively light wood, possesses,

undoubtedly, a higher transmissive power than any other in the list. Iron-wood, on the contrary, with a specific gravity of 1·426, stands low. Again, oak and Coromandel-wood—the latter so hard and dense, that it is used for sharp war-instruments by savage tribes—stand near the head of the list, while Scotch fir and other light woods stand low.

If we cast our eyes along the second and third columns of the table, we shall find that, in every instance, the velocity of propagation is greatest in a direction perpendicular to the ligneous layers. The law of molecular action, as regards the transmission of heat through wood, may therefore be expressed as follows:

At all points, not situate in the centre of the tree, wood possesses three unequal axes of calorific conduction, which are at right angles to each other. The first and principal axis is parallel to the fibre of the wood; the second and intermediate axis is perpendicular to the fibre, and to the ligneous layers; while the third and least axis is perpendicular to the fibre, and parallel to the layers.

MM. De la Rive and De Candolle have remarked upon the influence which its feeble conducting power in a lateral direction must exert, in preserving within a tree the warmth which it acquires from the soil. But Nature has gone farther, and clothes the tree with a sheathing of worse-conducting material than the wood itself, even in its worst direction. The following are the deflections, obtained by submitting a number of cubes of bark, of the same size as the cubes of wood, to the same conditions of experiment:

	Deflection	Corresponding deflection produced by the wood
Beech-tree bark	7°	10·8°
Oak-tree bark	7	11·0
Elm-tree bark	7	11·5
Pine-tree bark	7	12·0

The direction of transmission, in these cases, was from the interior surface of the bark outwards.

The average deflection, produced by a cube of wood, when the flux is lateral, may be taken at

12° ;

a cube of rock-crystal of the same size, produces a deflection of

90°.

There are the strongest experimental grounds for believing that rock-crystal possesses a higher conductive power than some of the metals.

The following numbers express the transmissive power of a few other organic structures:

Tooth of walrus	16
Tusk of East-Indian elephant	17
Whalebone	9
Rhinoceros horn	9
Cow's horn	9

The point is capable of still further illustration. Each of the substances mentioned in the following table being reduced to the cubical form, was submitted to an examination, similar in every respect to that of wood and quartz. While, however, a cube of the latter substance produces, as above stated, a deflection of 90°, a cube of

Sealing-wax produces a deflection of	0°
Sole leather	0
Bees'-wax	0
Glue	0
Gutta-percha	0
India-rubber	0
Filbert-kernel	0
Almond-kernel	0
Boiled ham-muscle	0
Raw veal-muscle	0

The substances here named are animal and vegetable

productions; and the experiments demonstrate the extreme imperviousness of every one of them. Starting from the principle, that sudden accessions or deprivations of heat are prejudicial to animal and vegetable health, we see that the materials chosen are precisely those best calculated to avert such changes.

I wish now to direct your attention to what may, at first sight, appear a paradoxical experiment. Placing a short prism of bismuth, and a similar one of iron, on the lid of a vessel containing hot water, the motion of heat propagates itself through both. The upper surface of each prism is coated with white wax, and you are to observe the melting of the wax. It is already beginning to yield, but on which? On the bismuth. How is this result to be reconciled with the fact, stated in our table, that, the conductivity of iron being 12, that of bismuth is only 2? In this experiment, the bismuth seems to be the best conductor. We solve this enigma by turning to our table at page 184, where we find that, the specific heat of iron being 0·1138, that of bismuth is only 0·0308. To rise, therefore, a certain number of degrees in temperature, iron requires more than three times the absolute quantity of heat required by bismuth. Thus, though the iron is really a much better conductor than the bismuth, and is at this moment accepting, in every unit of time, a much greater amount of heat than the bismuth, still, in consequence of the number of its atoms, or the magnitude of its interior work, the augmentation of temperature in its case is slow. Bismuth, on the contrary, can immediately devote a large proportion of the heat imparted to it, to the augmentation of temperature; and thus it apparently outstrips the iron, in the transmission of that motion to which temperature is due.

You see here, very plainly, the incorrectness of the statements sometimes made in books, and frequently also by candidates in our science examinations, regarding the experiment of Ingenhausz, already referred to. It is usually stated, that the greater the *quickness* with which the wax melts, the better is the conductor. If the bad conductor and the good conductor have the same specific heat, this is true; but in other cases, as proved by our last experiment, it may be entirely incorrect. The proper way of proceeding, as already indicated, is to wait until both the iron and the bismuth have attained a constant temperature—till each of them, in fact, has accepted, and is transmitting, all the thermal motion which it can accept, or transmit; when this is done, it is found that the quantity transmitted by the iron is six times greater than that transmitted by the bismuth.

These considerations also show that in our experiments on wood the quantity of heat transmitted by our cube in one minute's time, cannot, in strictness, be regarded as the expression of the conductivity of the wood, unless the specific heat of the various woods be the same. On this point, no experiments have been made. But, as regards the influence of molecular structure, the experiments hold good, for here we compare one direction with another, *in the same cube.* With respect to organic structures, I may add that, even allowing them time to accept all the motion which they are capable of accepting from a source of heat, their power of transmitting that motion is exceedingly low. They are really bad conductors.

WARMTH OF CLOTHES.

It is the imperfect conductivity of woollen textures which renders them so eminently fit for clothing.

They preserve the body from sudden accessions, and from sudden losses, of heat. The same quality of non-conductivity manifests itself, when we wrap flannel round a block of ice. The ice thus preserved is not easily melted. In the case of the human body, on a cold day, the woollen clothing prevents the transmission of motion from within outwards. In the case of the ice, on a warm day, the self-same fabric prevents the transmission of motion from without inwards. Animals which inhabit cold climates are furnished by Nature with their necessary clothing. Birds, especially, need this protection, for they are still more warm-blooded than the mammalia. They are furnished with feathers, and between the feathers the interstices are filled with down, the molecular constitution and mechanical texture of which render it, perhaps, the worst of all conductors. Here we have another example of that harmonious relation of life to the conditions of life, which is incessantly presented to the student of natural science.

The indefatigable Rumford made an elaborate series of experiments, on the conductivity of the substances used in clothing.[1] His method was this: A mercurial thermometer was fixed in the axis of a glass tube, ending in a globe, so that the bulb of the thermometer occupied the centre of the globe: the space between globe and bulb was filled with the substance whose conductive power was to be determined; the instrument was then heated to the temperature of boiling water, and afterwards plunged into a freezing mixture of pounded ice and salt, the times of cooling down 135° Fahr. being noted. They are recorded in the following table:

[1] Phil. Trans. 1792, p. 48.

Surrounded with	Seconds
Twisted silk	917
Fine lint	1032
Cotton wool	1046
Sheep's wool	1118
Taffety	1169
Raw silk	1264
Beaver's fur	1296
Eider down	1305
Hares' fur	1312
Wood ashes	927
Charcoal	937
Lampblack	1117

Among the substances here examined, hares' fur offered the greatest impediment to the transmission of the heat.

The transmission of heat is powerfully influenced by the mechanical state of the body through which it passes. The raw and twisted silk of Rumford's table illustrate this. Pure silica, in the state of hard rock-crystal, is a better conductor than bismuth or lead; but if the crystal be reduced to powder, the propagation of heat is exceedingly slow. Through transparent rock-salt heat is copiously conducted, through common table-salt very feebly. Asbestos is composed of certain silicates in a fibrous condition; I place some asbestos on my hand, and on it a red-hot iron ball. The ball can be thus held without inconvenience. That the division of the substance should interfere with the transmission might reasonably be inferred; for, heat being motion, anything which disturbs the continuity of the molecular chain, along which the motion is conveyed, must affect the transmission. In the case of the asbestos, the fibres are separated from each other by spaces of air; the motion has to pass from solid to air, and from air to solid. It is easy to see, that the transmission of vibratory motion through this composite texture must be very imperfect.

In the case of an animal's fur, this is more especially the case; for here, not only do spaces of air intervene between the hairs, but the hairs themselves, unlike the fibres of the asbestos, are very bad conductors. Lava has been known to flow over a layer of ashes, underneath which was a bed of ice, the non-conductivity of the ashes saving the ice from fusion. Red-hot cannon balls have been wheeled to the gun's mouth in wooden barrows partially filled with sand. Ice is packed in sawdust, to prevent it from melting; powdered charcoal is also an eminently bad conductor. But there are cases where sawdust, chaff, and charcoal, could not be used with safety, on account of their combustible nature. In such cases, powdered gypsum may be used with advantage. In the solid crystalline state, it is an incomparably worse conductor than silica, and it may be safely inferred, that, in the powdered state, its imperviousness far transcends that of sand, each grain of which is a good conductor. A jacket of gypsum powder, round a steam boiler, would materially lessen its loss of heat.

In percolating through the earth, water dissolves more or less of the substances with which it comes into contact. For example, in chalk districts, the water always contains a quantity of carbonate of lime; such water is called *hard water*. Sulphate of lime is also a common ingredient of water. In evaporating, the water only is driven off, the mineral is left behind, often in quantities too great to be held in solution. Many springs are strongly impregnated with carbonate of lime, and the consequence is, that when the waters of such springs reach the surface, and are exposed to the air, where they can partially evaporate, the mineral is precipitated, and forms incrustations on the surfaces of plants and stones, over which the water trickles. When water boils, the minerals are also, in great part, precipitated, and there is scarcely a kettle

in London, which is not internally coated with a mineral incrustation. This is an extremely serious difficulty, as regards steam boilers; the crust is a bad conductor, and it may become so thick, as materially to intercept the passage of heat to the water. Before you is a portion of a boiler belonging to a steamer, which was all but lost through the exhaustion of her fuel; to bring this vessel into port, not only her coals, but her spars, and every other piece of available wood, were burnt; the cause being this formidable incrustation, mainly carbonate of lime, which by its non-conducting power rendered a prodigal expenditure of fuel necessary. Doubtless, the slowness of many kettles in boiling would be found due to a similar cause.

One or two instances of the action of good conductors, in preventing the local accumulation of heat, will not be out of place here. These two spheres are of the same size, and are both covered closely with white paper. One of them is copper, the other is wood. I place a spirit-lamp underneath each of them. The motion of heat, of course, communicates itself to each ball, but in one, it is quickly conducted away from the place of contact with the flame, through the entire mass of the ball; in the other, this quick conduction does not take place, the motion therefore accumulates at the point where the flame plays upon the ball; and here you have the result. On turning up the wooden ball, the white paper is seen to be charred; the other ball, so far from being charred, is *wet* at its under surface by the condensation of the aqueous vapour generated by the lamp. Here is a cylinder covered closely with paper; I hold its centre, thus, over the lamp, turning it so that the flame shall play all round the cylinder: you see a well-defined mark, on one side of which the paper is charred, on the other side not. The cylinder is half brass and half wood, and this mark shows their

line of junction. Where the paper covers the wood, it is charred; where it covers the brass, it is not sensibly affected.

THE SAFETY LAMP.

The withdrawal of heat by a good conductor is strikingly illustrated by the action of wire gauze upon flame. Holding a piece of such gauze horizontally, I bring it down upon a tall gas-flame. You might imagine that the flame could readily pass through the open meshes of the gauze, but not a flicker passes (fig. 80). The combustion is entirely confined to the space under the gauze. I extinguish the flame, and allow the unignited gas to stream from the burner. The wire gauze being placed above the burner, the gas freely passes through the meshes. On igniting the gas above, we have the flame, but it

FIG. 80.

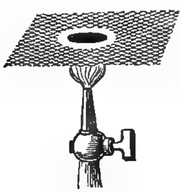
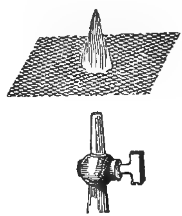


FIG. 81.



does not propagate itself downwards to the burner (fig. 81). Between the burner and the gauze is a space of four inches, filled with gas in a condition eminently favourable to ignition, but which does not ignite. This metallic gauze, then, which allows the gas to pass freely, intercepts the flame. And why? A certain temperature is necessary to cause the gas to burn; and by placing the wire gauze over the flame, or the flame over the wire gauze, the motion of that light and quivering thing is rapidly taken

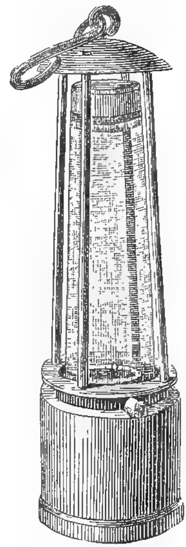
up by the comparatively heavy metal, which is a good conductor. The intensity of the molecular motion is so much lowered, that it is incompetent to propagate the combustion to the opposite side of the gauze. If the waste of motion could be avoided—if all the heat communicated to the gauze could be retained by the gauze—it would eventually rise to the temperature of the flame. The gauze, however, is continually wasting its heat by radiation and by contact with cool air; and the flame can heat it no farther than the point at which the waste, in a given time, is equal to the supply.

We are all, unhappily, too well acquainted with the terrible accidents that occur, through explosions in coal mines. You know that the cause of these explosions is the presence of a certain gas—a compound of carbon and hydrogen—generated in the coal strata. When this gas is mixed with a sufficient quantity of air, it explodes on ignition, the carbon of the gas uniting with the oxygen of the air, to produce carbonic acid; the hydrogen of the gas uniting with the oxygen of the air, to produce water. By the flame of the explosion, the miners are burnt; by the shock they are killed; but even should these not destroy life, they are often suffocated by the carbonic acid produced. The original gas is the miner's 'fire-damp,' the carbonic acid is his 'choke-damp' or 'after-damp.'

Sir Humphry Davy, after having assured himself of the action of wire gauze, just exhibited before you, applied it to the construction of a lamp, which should enable the miner to carry his light into an explosive atmosphere. He surrounded a common oil lamp by a cylinder of wire gauze (fig. 82). So long as this lamp is fed by pure air, the flame burns with the ordinary brightness of an oil flame; but when the miner comes into an atmosphere containing 'fire-damp,' his flame enlarges, and becomes less luminous. This enlargement of the flame ought to

be taken as a warning to retire. Still, though a continuous explosive atmosphere extends from the air outside, through the meshes of the gauze, to the flame within, ignition is not propagated across the gauze. A defect in the gauze, the destruction of the wire at any point by oxidation, would cause explosion. The rapid motion of the lamp through the air, or the impact of a 'blower' upon the lamp, might also force the flame through the meshes. In short, a certain amount of intelligence and caution is necessary in using the lamp. This intelligence, unhappily, is not always possessed, nor is this caution always exercised, and the consequence is, that even with the safety-lamp explosions still occur. Before permitting a man or boy to enter a mine, would it not be well to place these results, by experiment, visibly before him? Mere advice will not enforce caution; but let the miner have the physical image of what he is to expect, clearly and vividly before his mind, and he will find it a restraining force and a monitory influence, long after the effect of cautioning *words* has passed away.

FIG. 82.



A word or two, now, on the conductivity of liquids and gases. Rumford made numerous experiments on this subject, showing at once clearness of conception and skill of execution. He supposed liquids to be non-conductors, clearly distinguishing the 'transport' of heat, by convection, from true conduction; and in order to prevent convection in his liquids, he heated them at the top. In this way, he found the heat of a warm iron cylinder incompe-

tent to pass downwards, through 0·2 of an inch of olive oil ; he also boiled water in a glass tube, over ice, without melting the latter substance. The later experiments of M. Despretz apparently show that liquids possess true though extremely feeble, powers of conduction. Other experimenters have brought considerable skill to bear upon this exceedingly difficult subject. I say difficult on personal grounds, because many years ago I tried the subject of liquid conduction, and experienced its difficulty. Professor Guthrie has attacked it with consummate experimental skill. By means of an instrument devised by himself, and called a diathermometer, he has examined the 'specific thermal resistance' of twenty-three different liquids. The diathermometer consisted essentially of two metal cones, the one upright, the other inverted, between the horizontal bases of which was introduced a layer of the liquid to be examined. The base of the upper cone constituted the source of heat, which was kept at the required temperature by a continuous flow of warm water through the upper cone. The lower cone was filled with air which, expanded by the heat transmitted to it across the liquid layer, enabled the lower cone to play the part of an air thermometer. In all cases the circular edge of the liquid layer was free.[1] Water heads Professor Guthrie's list as the best conductor. He calls it 'the silver of liquids.' It is a remarkable result that alcohol, which so closely resembles water in chemical constitution, offers, according to Guthrie, nine times the resistance of water to the passage of heat.

The difficulty above referred to is illustrated by the discrepancies existing between Professor Guthrie and another highly skilled experimenter, Professor Beetz of Munich.

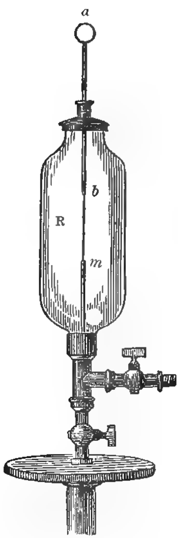
[1] The chill of vaporization from the free edge must have here come into play—a difficulty which may have prevented Professor Guthrie from examining ether and bisulphide of carbon.

Though not able to determine with accuracy the resistance of bisulphide of carbon, Professor Guthrie 'scarcely hesitates to predict that it has a great, perhaps the greatest, thermal resistance.' Professor Beetz, on the contrary, places the bisulphide at the head of liquid conductors. Professor Guthrie moreover makes water a twelve-times better conductor than chloroform. In one series of experiments, Professor Beetz makes chloroform a distinctly better conductor than water, while in a second series of higher temperatures, he finds that water has a slight, but only a slight, advantage. These and other discrepancies require explanation. Professor Beetz's method of experiment closely resembles that of Rumford; still the distinguished German experimenter found that jelly-like starch and pure water, when the temperatures are not high, conduct sensibly alike. This is a remarkable result, and it would be well worth while to inquire how Rumford reached a conclusion so entirely opposed to it.

The subject of gaseous conduction was taken up by the late Professor Magnus, and this distinguished philosopher thought his experiments proved that hydrogen gas conducts heat like a metal. The cooling action of air by convection may be thus illustrated On sending a voltaic current through a coil of platinum wire, it glows bright red. On stretching out the coil, so as to form a straight wire, the glow instantly sinks — you can hardly see it. This effect is due to the freer access of the cold air to the stretched wire. Here, again, is a receiver R (fig. 83) which can be exhausted at pleasure: attached to the bottom is a vertical metal rod, *m n*, and through the top another rod, *a b*, passes, which can be moved up and down through an air-tight collar, so as to bring the ends of the two rods within any required distance of each other. At present, the rods are united by two inches of platinum wire, *b m*, which may be

heated to any required degree of intensity by a voltaic current. On establishing connection with a small battery, the wire is barely luminous enough to be seen; in fact, the current from a single cell only is now sent through it. It is surrounded by air, which is carrying off a portion of its heat. When the receiver is exhausted, the wire glows more brightly than before. Allowing air to re-enter—the wire, for a time, is quite quenched, in fact, rendered perfectly black; but after the air has ceased to enter, its first feeble glow is restored. The current of air here passing over the wire, and destroying its glow, acts like the current established by the wire itself, through heating the air in contact with it. The cooling of the wire, in both cases, is due to convection, not to true conduction.

FIG. 83.



The same effect is obtained in a greatly increased degree, if hydrogen be used instead of air. We owe this interesting observation to Sir William Grove, and it formed the starting-point of Magnus's investigation. The receiver is now exhausted, the wire being almost white-hot. Air cannot do more than reduce that whiteness to bright redness; but observe what hydrogen can do. On the entrance of this gas, the wire is totally quenched; and even after the receiver has been filled with the gas, and the inward current has ceased, the glow of the wire is not restored. The electric current, now passing through the wire, is from two cells; I try three cells, the wire glows feebly; five cause it to glow more brightly, but even with five, it is but a bright red. Were no hydrogen there, the current now passing through the wire would

infallibly fuse it. Let us see whether this is not the case. On exhausting the receiver the effect of rarefaction soon begins to be visible. The wire whitens, and appears to thicken, until to those at a distance it seems as thick as a goose-quill. And now it glows, upon the point of fusion. A few additional strokes of the pump cause the light to vanish: the wire is fused.

This extraordinary cooling power of hydrogen has been usually ascribed to the mobility of its particles, which enables currents to establish themselves in this gas, with greater facility than in any other. But Professor Magnus conceives the chilling of the wire to be an effect of conduction. To impede, if not to prevent, the formation of currents, he passes his platinum wire along the axis of a narrow glass tube, filled with hydrogen. Although, in this case, the wire is surrounded by a mere film of the gas, and the presence of currents, in the ordinary sense, is scarcely to be assumed, the film shows itself just as competent to quench the incandescence as when the wire is caused to pass through a large vessel containing the gas. Professor Magnus also heated the closed top of a vessel, and found that the heat was conveyed more quickly from it to a thermometer, placed at some distance below the top, when the vessel was filled with hydrogen, than when it was filled with air. He found this to be the case even when the vessel was loosely filled with cotton wool or eider down. Here, he contends, currents could not be formed; the heat must be conveyed to the thermometer by the true process of conduction, and not by convection.

Ingenious as these experiments are, I do not think they establish the conductivity of hydrogen. Let us suppose the wire, in Magnus's first experiment, to be stretched along the axis of a wide cylinder containing hydrogen, we should have convection, in the ordinary sense, on heating the wire. Where does the heat thus

dispersed ultimately go? It is manifestly given up to the sides of the cylinder, and if we narrow our cylinder, we simply hasten the transfer. The process of narrowing may continue, till a narrow tube is the result—the convection between centre and sides will continue, and produce substantially the same cooling effect as before. The heat of the gas being instantly lowered, by communication to the heavy tube, it is prepared to re-abstract the heat from the wire. With regard, also, to the vessel heated at the top, it would require a surface mathematically horizontal, and a perfectly uniform application of heat to that surface—it would, moreover, be necessary to cut the heat sharply off, so as to prevent the least propagation down the sides of the vessel—to prevent convection. None of these conditions were secured. Even in the interstices of the eider down and cotton wool, the convective mobility of hydrogen will make itself felt, and, taking everything into account, I think the experimental question of gaseous conduction is still an open one.

LECTURE X.

COOLING A LOSS OF MOTION: TO WHAT IS THIS MOTION IMPARTED?—EXPERIMENTS ON SOUND BEARING ON THIS QUESTION—EXPERIMENTS ON LIGHT BEARING ON THIS QUESTION—THEORIES OF EMISSION AND UNDULATION—LENGTH OF WAVES AND NUMBER OF IMPULSES OF LIGHT—PHYSICAL CAUSE OF COLOUR—INVISIBLE RAYS OF THE SPECTRUM—THE CALORIFIC RAYS BEYOND THE RED—THE CHEMICAL RAYS BEYOND THE BLUE—DEFINITION OF RADIANT HEAT—REFLECTION OF RADIANT HEAT FROM PLANE AND CURVED SURFACES: LAWS THE SAME AS THOSE OF LIGHT—CONJUGATE MIRRORS.

RADIANT HEAT.

WE have this day reached the boundary of one of the two great divisions of our subject. Hitherto we have dealt with heat, while associated with solid, liquid, or gaseous bodies. We have found it competent to produce changes of volume in all these bodies. We have also observed it reducing solids to liquids, and liquids to vapours; we have seen it transmitted through solids, by the process of conduction, and distributing itself through liquids and gases by the process of convection. We have now to follow it into conditions of existence, different from any which we have heretofore examined.

This heated copper ball hangs in the air; you see it glow, the glow sinks, the ball becomes obscure; in popular language, the ball cools. Bearing in mind what has been said on the nature of heat, we must regard this cooling as a loss of molecular motion. But to be lost the motion must be imparted to something; to what then is the molecular motion of this ball transferred? You

would, perhaps, answer, to the air; and this is partly true: over the ball air is passing, and rising in a heated column, which is quite visible against the screen when we allow the electric beam to pass through the warmed air. But not the whole, not even the chief part, of the molecular motion of the ball is dissipated in this way. If the ball were placed *in vacuo*, it would still cool. Rumford, of whom we have heard so much, contrived to hang a small thermometer, by a single fibre of silk, in the middle of a glass globe, exhausted by means of mercury; and he found that the calorific rays passed from without, across the vacuum, to the thermometer, thus proving the transmission of the heat to be independent of air. Davy, with an apparatus here present, showed that the heat from the carbon-points of the Voltaic arc passes freely through an air-pump vacuum; and we can repeat his experiment substantially for ourselves. It is only necessary to take the receiver already employed (fig. 83, p. 266), and, removing the remains of the platinum wire then destroyed, attach to the ends of the two rods, *b* and *m*, two bits of retort-carbon, and exhaust the receiver. Bringing the carbon-points together, and sending a current across them, the moment they are drawn a little apart, the electric light shines forth, while the thermo-electric pile is at hand, ready to receive a portion of the rays. The needle of the galvanometer connected with the pile at once flies aside, and this has been accomplished by heat-rays which have crossed the air-vacuum.

But if not to the air, to what is the motion of our cooling ball communicated? We must reach by easy stages the answer to this question. It was a very considerable step in science to obtain a clear conception of the way in which sound is transmitted through air, and a very important experiment was made by Robert Boyle, and after him by Hauksbee, when they showed that sound

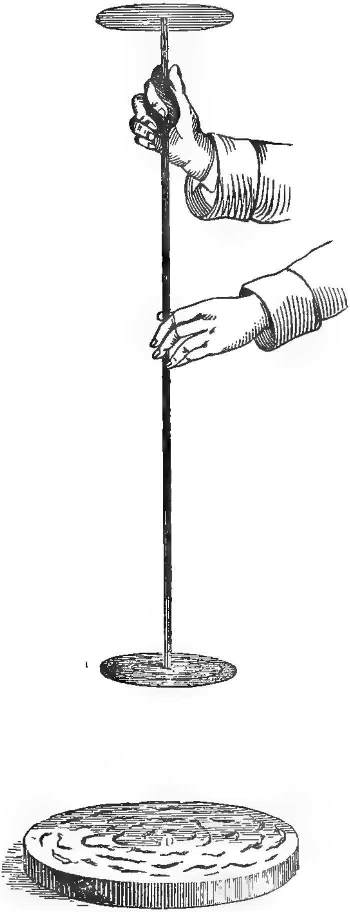
could not propagate itself through a vacuum. I wish to make manifest to you the conveyance of the vibrations of sound by the air, and employ for the purpose a flat bell, turned upside-down, and supported by a stand. When a fiddle-bow is drawn across the edge of the bell you hear its tone; the bell is now vibrating, and when sand is thrown in it arranges itself upon the bottom so as to form a definite figure. If the bell were filled with water, we should have the surface fretted with beautiful crispations. These would show that the bell, in emitting this note, divides itself into four swinging parts, separated from each other by lines of no swinging. When a sheet of tracing-paper, drawn tightly over a hoop, so as to form a kind of fragile drum, is passed round the bell, the membrane bursts into a musical roar when brought within half an inch of the vibrating surface. The motion of the bell, communicated to the air, has been transmitted to the membrane, and the latter is thus converted into a sonorous body.

The two plates of brass, A B (fig. 84), are united together by a metal rod. The plates have been darkened by bronzing, and on both of them is strewn a quantity of fine white sand. I take the connecting brass rod by its centre, between the finger and thumb of my left hand, and holding it upright, draw, with my right, a piece of flannel, over which a little powdered resin has been shaken, along the rod. You hear the sound; but observe the behaviour of the sand: a single stroke has caused it to jump into a series of sharply defined concentric rings visible to you all. The vibrations here imparted to the rod have communicated themselves to both the disks, and divided each of them into a series of vibrating segments, separated from each other by lines of no vibration, on which the sand finds peace.

The transmission of these vibrations, from the lower disk through the air, is now to be rendered evident. On

the floor is a paper drum, D, with dark-coloured sand strewn uniformly over it; I might stand on the table,—or indeed as high as the ceiling, and produce the effect which you are now to witness. Pointing the rod which unites the plates, in the direction of the paper drum, I draw the resined rubber vigorously over the rod: a single stroke has caused the sand on the drum to spring into a reticulated pattern. A precisely similar effect is produced, by sound, on the drum of the ear; the tympanic membrane is caused to shiver, like that drum-head of paper, and its motion, conveyed to the auditory nerve, and transmitted thence to the brain, awakes in us the sensation of sound.

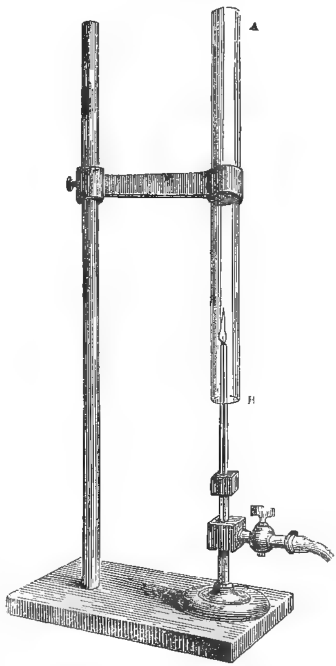
Fig. 84.



A still more striking example of the conveyance of the motion of sound through air may be brought before you. By permitting a jet of gas to issue through a small orifice, a slender flame is obtained, and by turning a cock, the flame is reduced to a height of about half an inch. It is then introduced into

a glass tube, A B (fig. 85), twelve inches long. Give me your permission to address that flame. If I be skilful enough to pitch my voice to the proper note, the flame will respond by suddenly sounding a note of the same pitch, and it will continue singing, as long as the gas continues to burn. The burner is now arranged within the tube, which covers it to a depth of a couple of inches. If the tube were lower, the flame would sing of its own accord, as in the well-known case of the hydrogen harmonica; but, with the present arrangement, it cannot sing until ordered to do so. I make a preliminary trial of my voice upon the flame. It does not respond, because it has not been spoken to in the proper tone. But a note of somewhat higher pitch causes it to stretch its tongue and sing vigorously. When the proper pitch has been ascertained, the experiment is sure to succeed, and from a distance of twenty or thirty feet, the flame, when sung unto, is caused to sing responsively. With a little practice, moreover, one is able to command a flame to sing and to stop singing, while it strictly obeys the injunction. Here, then, we have a striking example of the conveyance of sonorous vibrations through air, and of their communication to a body eminently sensitive to their action.

FIG. 85.



THE LUMINIFEROUS ETHER.

Why are these experiments on sound performed? Simply for the purpose of giving you clear conceptions regarding what takes place in the case of heat; to lead you from the tangible to the intangible; from the region of sense into that of theory.

After philosophers had become aware of the manner in which sound was produced and transmitted, analogy led some of them to suppose that light might be produced and transmitted in a somewhat similar manner. And perhaps, in the whole history of science, there was never a question more hotly contested than this one. Sir Isaac Newton, as indicated in our second lecture, supposed light to consist of minute particles, darted out from luminous bodies. Huyghens, the contemporary of Newton, found great difficulty in admitting this cannonade of particles; or in realising that they could shoot with inconceivable velocity through space, and yet not disturb each other. This celebrated man entertained the view that light was produced by vibrations, similar to those of sound. Euler supported Huyghens, and one of his arguments, though not truly physical, is so quaint and curious, that I will repeat it here. He considers our various senses, and the manner in which they are affected by external objects. 'With regard to smell,' he says, 'we know that it is produced by material particles, which issue from a volatile body. In the case of hearing, nothing is detached from the sounding body, and in the case of feeling we must touch the body itself. The distance at which our senses perceive bodies is, in the case of touch, no distance; in the case of smell, a small distance; in the case of hearing, a considerable distance; but, in the case of sight, greatest of all. It is therefore more probable that the

same mode of propagation is common to sound and light than to odours and light;—that luminous bodies should behave, not as volatile bodies, but as sounding ones.'

The authority of Newton bore these men down, and not until a man of genius within these walls took up the subject, had the Theory of Undulation any chance of coping with the rival Theory of Emission. To Dr. Thomas Young, formerly Professor of Natural Philosophy in the Royal Institution, belongs the honour of stemming this tide of authority, and of establishing, on a safe basis, the Undulatory Theory of light. Great things have been done in this edifice; but scarcely a greater thing than this. And Young was led to his conclusion regarding light, by a series of investigations on sound. He, like ourselves at the present moment, rose from the known to the unknown, from the tangible to the intangible. This subject has been illustrated and enriched by the labours of genius ever since the time of Young; but one name only will I here associate with his,—a name which, in connection with this question, can never be forgotten: that is, the name of Augustin Fresnel.

According to the theory now universally received, light consists of a vibratory motion of the atoms and molecules of the luminous body; but how is this motion transmitted to our organs of sight? Sound has the air as its medium, and a close examination of the phenomena of light has led philosophers to the conclusion, that space is occupied by a substance almost infinitely elastic, through which the pulses of light make their way. Here your conceptions must be perfectly clear. It is just as easy to picture a vibrating atom as to picture a vibrating cannon-ball; and there is no more difficulty in conceiving of this *ether*, as it is called, which fills space, than in imagining all space filled with jelly.[1] The atoms of

[1] We have no means of producing an ether-vacuum.

luminous bodies vibrate, and you must figure their vibrations, as communicated to the ether, being propagated through it in pulses or waves; these waves enter the pupil of the eye, cross the ball, and impinge upon the retina, at the back of the eye. The motion thus communicated to the retina, is transmitted thence along the optic nerve to the brain, and there announces itself to consciousness, as light.

On the screen in front of you is projected an image of our incandescent carbon-points. The points are first brought together, and then separated. You notice first the place of contact rendered luminous, then you see the glow conducted downwards, to a certain distance along the rod of carbon. This, as you know, is in reality the conduction of motion. When the circuit is interrupted, the points continue to glow for a short time. Their light is now subsiding, and now they are quite dark; but at the present moment, there is a copious emission from these points, which, though incompetent to affect sensibly the nerves of vision, is quite competent to affect other nerves of the human system. To the eye of the philosopher, looking at such matters without reference to sensation, these obscure radiations are substantially the same in kind as those which produce the impression of light. You must, therefore, figure the molecules of the heated body as in a state of motion; you must figure that motion as communicated to the surrounding ether, and transmitted through it with a velocity which we have the strongest reason for believing to be the same as that of light. When, therefore, you turn towards a fire on a cold day and expose your chilled hands to its influence, the warmth which you feel is due to the impact of these ethereal billows upon your skin. They throw the nerves into motion, and the consciousness, corresponding to this motion, is what we popularly call warmth. Our task,

during the lectures which remain to us, will be to examine the laws and properties of heat thus propagated through the ether, in which form it is called *Radiant Heat.*

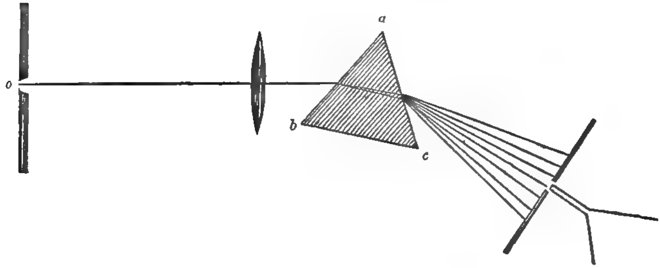
For the investigation of this subject, we possess our invaluable thermo-pile, the face of which is now coated with lampblack, a powerful absorber of radiant heat. Holding the instrument before my cheek, which is a radiating body, the pile drinks in the rays. They generate electricity, and the needle of the galvanometer moves up to 90°. Withdrawing the pile from the source of heat, and allowing the needle to come to rest, I place a slab of ice in front of the pile. A deflection in the opposite direction follows, as if rays of cold were striking on the instrument. Rumford indeed maintained with great tenacity the existence of 'frigorific rays.' But in this case the pile is really the hot body which radiates against the ice; the face of the pile is thus chilled, and the needle moves up to 90° on the side of cold. Our pile is, therefore, not only available for the examination of heat communicated to it by direct contact, but also for the examination of radiant heat. Let us apply it at once to a most important investigation, and examine, roughly in the first instance, the distribution of heat in the electric spectrum.

HEAT OF SPECTRUM.

The spectrum is formed by sending a slice of pure white light from the slit, *o* (fig. 86), through a double convex lens, and then through a prism, *a b c*, built up of plane glass sides, and filled with the liquid bisulphide of carbon. This liquid gives a richer display of colour than glass does, and this is one reason for its employment in preference to glass. The white beam is now reduced to

its component colours—red, orange, yellow, green, and blue; the long blue space being usually subdivided into blue, indigo, and violet. I will now cause a thermo-pile

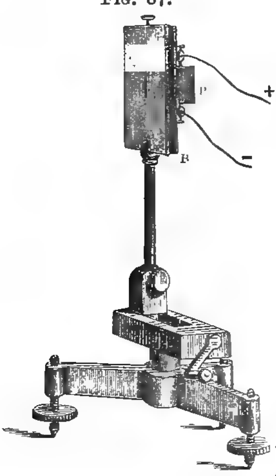
Fig. 86.



of a particular construction to pass gradually through all these colours in succession, so as to test their heating powers, and you shall observe the consequent action on the galvanometer.

The experiment is made with a beautiful piece of apparatus (fig. 87), designed by Melloni, and executed, with his accustomed skill, by Ruhmkorff. It consists of a polished brass plate, A B, attached to a stem, mounted on a horizontal bar, which, by means of a screw, may have motion imparted to it. By turning an ivory handle in one direction, the plate of brass is caused to approach; by turning it in the other, it is caused to recede, the motion being so fine and gradual, that we can, with ease and certainty, push the screen through a space less than $\frac{1}{2000}$th of an inch. You observe a narrow vertical slit in the

Fig. 87.



middle of the plate A B, and something dark behind it. That dark line is the blackened face of a thermo-electric pile, P, the elements of which are ranged in a single row, and not in a square, as in our other instrument. We will allow distinct slices of the spectrum to fall on that slit · each wil. impart whatever heat it possesses to the pile, and the quantity of heat will be marked by the needle of our galvanometer.

At present, a small but brilliant spectrum falls upon the plate A B, but the pile is quite out of the spectrum. On turning the handle, the slit gradually approaches the violet; the light now falls upon it, but the needle does not move sensibly. In the indigo the needle is still quiescent; the blue also shows no action. Nor does the green. The yellow falls upon the slit, and the motion of the needle is now perhaps for the first time visible to you; but the deflection is small, though the pile is exposed to the most luminous part of the spectrum.[1] I pass on to the orange, which is less luminous than the yellow, but you observe, though the light diminishes, the heat increases; the needle moves still farther. In the red, finally, which is less luminous than the orange, we have the greatest thermal power of the visible spectrum.

I now cause the pile to pass entirely out of the spectrum, quite beyond the extreme red. The needle goes promptly up to the stops. So that we have here a heat-spectrum which we cannot see, and whose thermal power is far greater than that of any part of the visible spectrum. In fact, the electric light, with which we deal, emits an infinity of rays, converged by our lens, refracted by our prism, forming the prolongation of our spectrum, but utterly incompetent to excite the optic nerve. It is the same with the sun. Our orb is rich in these obscure rays;

[1] I am here dealing with a large lecture-room galvanometer.

and though they are, for the most part, cut off by our atmosphere, multitudes of them still reach us. To the illustrious William Herschel we are indebted for this discovery. I propose, in a future lecture, to sift the composite emission of our electric lamp, detaching the visible from the invisible rays, and illustrating the discoveries which have been recently made in connection with the subject of obscure radiation.

The visible spectrum, then, simply marks an interval in which the rays are so related to our organisation, as to excite the impression of light. Beyond this interval, *in both directions,* obscure rays fall; those falling beyond the red being powerful to produce heat, while those falling beyond the violet are powerful to promote chemical action. How then are we to picture to ourselves the rays, visible and invisible, which fill the space occupied by the spectrum? Observe first, that the entire beam of white light is drawn aside or refracted by the prism, but the violet is pulled aside more than the indigo, the indigo more than the blue, the blue more than the green, the green more than the yellow, the yellow more than the orange, and the orange more than the red. The colours are differently refrangible, and upon this depends the possibility of their separation. To every particular degree of refraction belongs a definite colour, and no other. But why should light of one degree of refrangibility produce the sensation of red, and light of another degree the sensation of green? This leads us to consider more closely the cause of these sensations.

A reference to the phenomena of sound will materially help us here. Figure clearly to your minds a harp-string vibrating to and fro; it advances and causes the particles of air in front of it to crowd together, thus producing a *condensation* of the air. It retreats, and the air particles behind it separate more widely, thus producing a *rare-*

faction of the air. The string again advances and produces a condensation as before, it again retreats and produces a rarefaction. In this way, the air, through which the sound of the string is propagated, is moulded into a regular sequence of condensations and rarefactions, which, at the freezing temperature, travel with a velocity of about 1,100 feet a second.

The condensation and rarefaction constitute what is called a sonorous pulse or *wave.* The length of the wave is measured from the centre of one condensation to the centre of the next one. Now, the quicker a string vibrates, the more quickly will these pulses follow each other, and the shorter, at the same time, will be the length of each individual wave. Upon these differences the *pitch* of a note in music depends. If a violin player wishes to produce a note of higher pitch, he shortens the string by pressing his finger on it; thereby augmenting the rapidity of vibration. If his point of pressure exactly halves the length of his string, he obtains the *octave* of the note which the string emits, when vibrating as a whole. Boys are chosen as choristers to produce the shrill notes, men to produce the bass notes; the reason being, that the boy's vocal chords vibrate more rapidly than the man's. So, also, the hum of a gnat is shriller than that of a beetle, because the smaller insect can send a greater number of impulses per second to the ear.

PHYSICAL CAUSE OF COLOUR.

We have now cleared our way towards a full apprehension of the physical cause of colour. This spectrum is to the eye what the musical scale is to the ear; its different colours represent notes of different pitch. The vibrations which produce the impression of red are slower,

and the ethereal waves which they generate are longer, than those which produce the impression of violet, while the other colours are excited by waves of intermediate lengths. The lengths of the waves both of sound and light, and the number of shocks which they respectively impart to the ear and eye, have been strictly determined. Let us here go through a simple calculation. Light travels through space at a velocity of 186,000 miles a second. Reducing this to inches, we find the number to be 11,784,960,000. Now it is found that 39,000 waves of red light, placed end to end, would make up an inch; multiplying the number of inches in 186,000 miles by 39,000 we obtain the number of waves of red light embraced in a distance of 186,000 miles: this number is 459,613,440,000,000. *All these waves enter the eye in a single second.* To produce the impression of red, the retina must be hit at this inconceivable rate. To produce the impression of violet, a still greater number of impulses is necessary. It would take 57,500 waves of violet to fill an inch, and the number of shocks required to produce the impression of this colour amounts to six hundred and seventy-eight millions of millions per second. The other colours of the spectrum, as already stated, rise gradually in pitch from red to violet. Beyond the violet we have rays of too high a pitch to be visible, while beyond the red we have rays of too low a pitch to be visible. Both as regards light and sound, our organs embrace a certain practical range, beyond which, on both sides, though the objective cause exists, our nerves cease to be influenced by it.

When, therefore, this red-hot copper ball is placed before you, and you watch the waning of its light, you will have a perfectly clear conception of what is occurring. The atoms of the ball oscillate in a resisting medium, which accepts their motion and transmits it on all sides with incon-

ceivable velocity. The oscillations competent to produce light are soon exhausted; the ball becomes dark, still its atoms oscillate, sending through the ether waves of greater length and slower period than those appropriate to vision. The ball cools as it thus loses its atomic motion, but no cooling to which it can be practically subjected can entirely deprive it of such motion. In other words, all bodies, whatever may be their temperature, are radiating heat. From every person here present, waves are speeding away, some of which strike this cooling ball, and restore a portion of its lost motion. But the motion thus received by the ball is far less than that which it expends, and the difference expresses the ball's loss of heat. As long as this state of things continues, the ball will continue to show an ever-lowering temperature: its temperature will sink until the quantity it emits is equal to the quantity which it receives, and at this point its temperature becomes constant. Thus, though you are conscious of no reception of heat when you stand before a body of your own temperature, an interchange of rays is passing between you and it. Every superficial atom of each mass is sending forth its waves, which cross those that move in the opposite direction, every wave asserting its own individuality amid the entanglement of its fellows. When the sum of motion received is greater than that given out, warming is the consequence; when the sum of motion given out is greater than that received, chilling takes place. This is Prevost's Theory of Exchanges, expressed in the language of the Wave Theory of Heat.

REFLECTION OF HEAT.

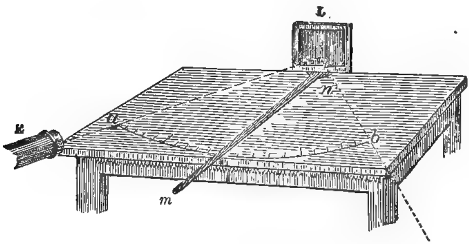
You observed when the thermo-electric pile was placed in front of my cheek there was attached to it an open cone which was not employed in our former experiments. This cone is silvered inside, and it is intended to augment the action of feeble radiations, by converging them upon the face of the thermo-pile. It does this by reflection. Instead of shooting wide of the pile, as many of them would do if the reflector were removed, the rays meet the silvered surface, and glance from it against the pile. The augmentation of the effect may be thus shown. I place the pile at one end of the table without its reflector, and at a distance of four or five feet a copper ball, hot—but not red-hot; you observe scarcely any motion of the needle of the galvanometer. Disturbing nothing, I now attach the reflector to the pile; the needle instantly goes up to 90°, declaring the augmented action.

The law of this reflection is precisely the same as that of light. A few minutes may be usefully devoted to the illustration of this subject. Observe this apparently solid cylindrical beam, issuing horizontally from our electric lamp, and marking its track thus vividly upon the dust of the darkened room. Permitting the beam to fall upon a plane mirror, it is reflected, and it now strikes the ceiling. The horizontal beam here is the *incident* beam, the vertical one is the *reflected* beam, and the law of light, as many of you know, is, that the angle of incidence is equal to the angle of reflection. The incident and reflected beams now enclose a right angle, and when this is the case we may be sure that each beam encloses, with a perpendicular to the surface of the mirror, an angle of 45°.

I place the electric lamp at the corner, E, of the table (fig. 88), behind which is fixed a small looking-glass, L;

and on which is drawn a large arc, *a b*. Attached to the mirror is a long straight lath, *m n*, while the mirror, resting upon rollers, can be turned by the lath, which is to serve as an index. Those directly in front may see that the lath itself and its reflection in the looking-glass form a straight line, which proves that the lath is perpendicular to the reflecting surface. Right and left of the central line, *m n*, the arc is divided into ten equal

FIG. 88.



parts; that is to say, commencing at the end *a* with 0, the arc is graduated up to 20. I now turn the lath index, so that it shall be in the line of the beam emitted by the lamp. The beam falls upon the mirror, striking it as a perpendicular, and is reflected back from it along the line of incidence. I now move the index to 1; the reflected beam draws itself along the table, cutting the figure 2. Moving the index to 2, the beam cuts the figure 4; moving the index to 3, the beam falls on 6; moving it to 5, the beam falls on 10; moving it to 10, the beam falls on 20. We have here demonstrated two important laws; first, that the angle of incidence is equal to the angle of reflection, and secondly, that the reflected beam moves twice as fast as the index. This is usually expressed by the statement, that the angular velocity of a reflected beam is twice that of the mirror which reflects it.

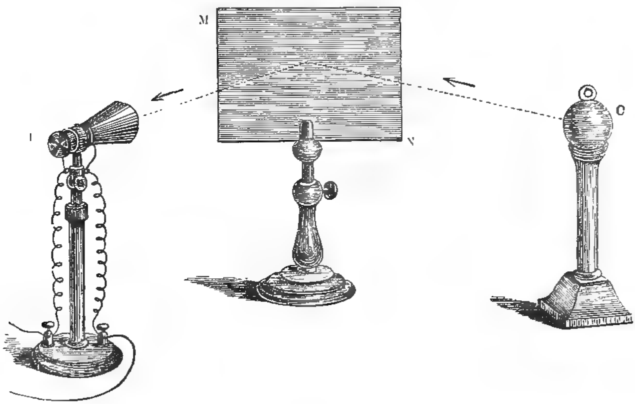
You have already learnt that the incandescent carbon-points emit an abundance of obscure rays—rays of pure heat, which have no illuminating power. We have now to learn that these rays obey precisely the same laws as the rays of light. I have here a selected piece of glass, so black and opaque that when you look through it at the electric light, or even at the noonday sun, you see nothing. It cuts off every ray of light; but, strange as it may appear to you, it is, to some extent, transparent to the obscure rays of the lamp. I extinguish the light by interrupting the current, interpose my black glass in front of the electric lamp, and place my thermo-pile on the table at the number 20, where the luminous beam fell a moment ago. The pile is connected with the galvanometer, and the needle of the instrument is now at zero. On igniting the lamp, no light makes its appearance, but the needle of the galvanometer swings to 90°, through the action of the non-luminous rays upon the pile. When the pile is moved right or left from its present position, the needle immediately sinks to zero. Here the calorific rays have pursued the precise track of the luminous rays. Repeating the experiments already executed with light,—bringing the index in succession to 1, 2, 3, 5, and 10, it may be proved that, in the case of radiant heat also, the angular velocity of the reflected beam is twice that of the mirror.

In these experiments the heat is, or has been, associated with light. But it may be shown that the law holds good for rays emanating from a truly obscure body. I set a copper ball, warm, but dark, upon a candlestick C (fig. 89), as a support, and place the pile, P, with its conical reflector turned away from C, so that no *direct* ray from the ball can reach the pile. The galvanometer needle remains at zero. I now introduce the tin reflector, M N, so that a line drawn to it from the ball shall make the same angle with a perpendicular to the reflector, as a

line drawn from the pile. True to the law, the heat-rays emanating from the ball rebound from it, strike the pile, and produce a prompt motion of the needle.

Like the rays of light, the rays of heat emanating from our ball proceed in straight lines through space, diminishing

FIG. 89.

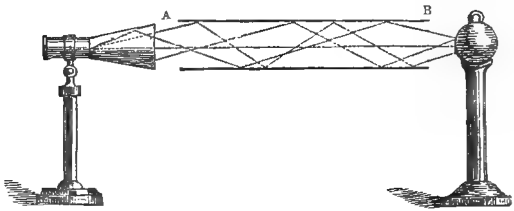


in intensity, exactly as light diminishes. Thus, this ball, which when close to the pile causes the needle of the galvanometer to fly up to 90°, produces at a distance of 4 feet 6 inches a scarcely sensible action. Its rays are squandered on all sides, and comparatively few of them reach the pile. But I now introduce between the pile and the ball a tin tube, A B (fig. 90), four feet long. It is polished within, and therefore capable of reflection. The calorific rays strike the interior surface obliquely, are reflected from side to side of the tube, and thus enabled to reach the pile. The needle, which a moment ago showed no sensible action, moves promptly to its stops.

These experiments illustrate sufficiently the reflection of radiant heat by plane surfaces; let us turn for a

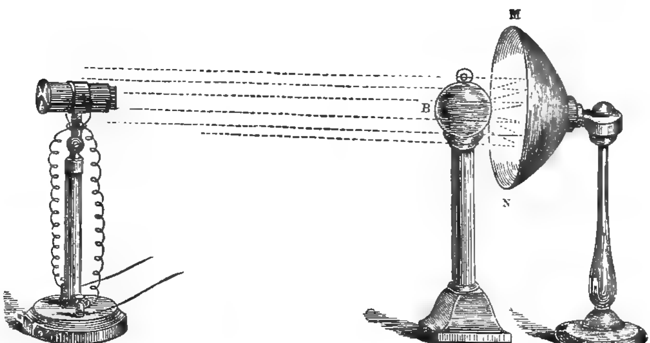
moment to reflection from curved surfaces. The concave mirror, M N (fig. 91), is formed of copper, coated with silver. The warm copper ball, B, is placed at a

FIG. 90.



distance of eighteen inches from the pile, whose conical reflector is now removed. Unaided by the mirror the rays from the ball produce scarcely any motion of the needle. If the reflector, M N, were placed properly behind

FIG. 91.



a candle, its rays would be collected, and sent back in a cylinder of light. In the same manner the mirror collects and reflects the calorific rays emitted by the ball B. You cannot, of course, see the track of these obscure rays; but

the galvanometer reveals the action, the needle of the instrument going promptly up to 90°.

CONJUGATE MIRRORS.

The action is vastly intensified by this pair of larger mirrors, which have a historic interest for me. One of them is placed flat upon the table. The curvature of this mirror is so regulated, that if a light be placed at its focus, the rays which fall divergent upon the curved surface are reflected upwards from it parallel. In the focus I place our carbon-points, bring them into contact, and then draw them a little apart; the electric light flashes against the mirror, a vertical beam, marked by the shining dust of the room, rising upwards from the reflector. If we reversed the experiment, and allowed a parallel beam to fall upon the mirror, the rays of that beam, after reflection, would be collected in the focus. To make this experiment we introduce the second mirror, which is suspended from the ceiling. Drawing it up to a height of 20 or 25 feet, the vertical beam, which previously fell upon the ceiling, is now received by the reflector. In the focus of the upper mirror is hung a bit of oiled paper, and you observe how intensely that piece of paper is illuminated, not by the direct light from below, but by the reflected light converged upon it by the mirror above.

Some of you have probably witnessed the extraordinary action of light upon a mixture of hydrogen and chlorine, and we may now exhibit this action in a striking way. A transparent collodion balloon is filled with the mixed gases. Lowering the upper reflector, the balloon is suspended from a hook attached to it, so that the little globe swings in the focus. We will now draw the mirror quite up to the ceiling (fig. 92). Placing, as before, the

Fig. 92.

carbon-points in the focus of the lower mirror, the moment they are drawn apart, the gases explode. And remember, this is the action of the *light*. You know that collodion is an inflammable substance, and hence might suppose it to be the *heat* of the coke-points which ignites it, and that it ignites the gases. But the flakes of the balloon descend on the table, proving that the luminous rays went harmlessly through it, caused the gases to explode, the hydrochloric acid, formed by their combustion, having preserved the inflammable envelope.

In the focus of the upper mirror is now placed a second balloon, containing a mixture of oxygen and hydrogen, on which light has no sensible effect. In the focus of the lower one is

placed a red-hot copper ball. The calorific rays are now reflected and converged, as the luminous ones were reflected and converged in the last experiment; but they act upon the envelope, which has been purposely blackened to enable it to intercept the heat-rays. Explosion occurs, but you now see no trace of the balloon; the inflammable substance is entirely dissipated.

Let us lower the upper mirror once more, and suspend in its focus a flask of hot water. The thermo-pile is now placed at the focus of the lower mirror. Its face being turned upwards, and exposed to the direct radiation of the warm flask, there is no sensible action produced by the direct rays. But when the face of the pile is turned downwards, if light and heat behave alike, the rays from the flask which strike the reflector will be collected at its focus. You see that this is the case; the needle, which was not sensibly affected by the direct rays, goes up to its stops. The direction of that deflection is to be noted; the red end of the needle moves towards you.

In the place of the flask of hot water, I now suspend a second one containing a freezing mixture. Placing, as in the former case, the pile in the focus of the lower mirror; when turned directly towards the upper flask, there is no action. Turned downwards, the needle moves, the red end coming towards me.

Does it not appear as if this body in the upper focus were now emitting rays of cold, which are converged by the lower mirror, like the rays of heat in our former experiment? The facts are exactly complementary, and it would seem that we have precisely the same right to infer, like Rumford, from this experiment the existence and convergence of cold-rays, as we have from the last experiment to infer the existence and convergence of heat-rays. Many of you, no doubt, have already perceived the real

state of the case. The pile is a warm body, but in the last experiment, the heat which it lost by radiation was more than made good by that received from the hot flask above. *Now* the case is reversed; the quantity which the pile radiates is in excess of the quantity which it receives, and hence the pile is chilled;—the exchanges are against it, its loss of heat is only partially compensated—and the deflection due to cold is the necessary consequence.[1]

[1] The vertical arrangement of the conjugate mirrors is due to Davy, who also suspended an iron cage with burning charcoal in the focus of the upper mirror. In an excellent magazine, published in Glasgow when I was a youth, I first read an account of Davy's experiments; and to the present hour I remember the yearning which took possession of me to be, like him, a natural philosopher. I had little notion at the time that I should ever work with the very instruments the description of which had so fired my young enthusiasm.

LECTURE XI.

LAW OF DIMINUTION WITH THE DISTANCE—THE WAVES OF SOUND LONGITUDINAL; THOSE OF LIGHT TRANSVERSAL—WHEN THEY OSCILLATE, THE MOLECULES OF DIFFERENT BODIES COMMUNICATE DIFFERENT AMOUNTS OF MOTION TO THE ETHER—RADIATION THE COMMUNICATION OF MOTION TO THE ETHER; ABSORPTION THE ACCEPTANCE OF MOTION FROM THE ETHER—THOSE SURFACES WHICH RADIATE WELL ABSORB WELL—A CLOSE WOOLLEN COVERING FACILITATES COOLING—PRESERVATIVE INFLUENCE OF GOLD-LEAF—TRANSPARENCY AND DIATHERMANCY—DIATHERMIC BODIES BAD RADIATORS—DEFINITION OF THE TERM 'QUALITY' AS APPLIED TO RADIANT HEAT—THE RAYS WHICH PASS WITHOUT ABSORPTION DO NOT HEAT THE MEDIUM—PROPORTION OF LUMINOUS TO OBSCURE RAYS IN VARIOUS FLAMES.

LAW OF INVERSE SQUARES.

THE intensity of radiant heat diminishes with the distance, in the same manner as that of light. What then is the law of diminution for light? An extremely simple experiment will sufficiently answer this question. Each side of this square sheet of drawing-paper measures two feet; I fold it up so as to form a smaller square, with sides one foot in length. The carbon-points of the electric lamp are now sixteen feet from the screen; and at a distance of eight feet, that is, exactly midway between the screen and the points, I hold this square of paper. From the points the rays, uninfluenced by any lens, are emitted in straight lines, the square of paper casting a well-defined shadow on the screen. Let us mark the boundary of that shadow, and then unfold the sheet of paper, so

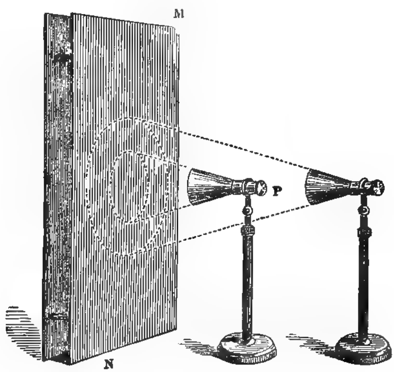
as to obtain the original large square. You see, by the creases, that it is exactly four times the area of the smaller one. Placed against the screen, this large sheet exactly covers the space occupied a moment ago by the shadow of the small square.

On the small square, therefore, when it stood midway between the lamp and screen, a quantity of light fell which, when the small square is removed, is diffused over four times the area upon the screen. But if the same quantity of light is diffused over four times the area, it must be diluted to one-fourth of its original intensity. Hence, by doubling the distance from the source of light, we diminish the intensity to one-fourth. By a precisely similar mode of experiment, we cou'd prove, that, by trebling the distance, we diminish the intensity to one-ninth; and by quadrupling the distance we reduce the intensity to one-sixteenth: in short, we thus demonstrate the law that the intensity of light diminishes, as the square of the distance increases. This is the celebrated law of Inverse Squares, as applied to light.

It has just been stated that heat diminishes according to the same law. We will now approach the proof of this through an apparent refutation. This narrow tin vessel, M N (fig. 93), which is coated outside with lampblack, presents a side a square yard in area. The vessel is filled with hot water, which converts its large surface into a source of radiant heat. I now place the conical reflector on the thermo-pile, P, but instead of permitting it to remain a reflector, I push into the hollow cone a lining of black paper, which fits exactly, and which, instead of reflecting any heat that may fall obliquely on it, effectually cuts off the oblique radiation. The pile is now connected with the galvanometer, and its cone is close to the radiating surface, the face of the pile itself being about six inches distant from the surface.

The needle of the galvanometer moves, and comes to rest at 60°, where it would remain as long as the temperature of the radiating surface continues sensibly constant. I now gradually withdraw the pile from the surface, and ask you to observe the effect upon the galvanometer. You might naturally expect that as the pile is withdrawn, the intensity of the heat will diminish, and that the deflection of the galvanometer will fall in a corresponding degree. The pile is now at double the distance, but the needle does not move; at treble the distance, the needle is still sta-

FIG. 93.



tionary; we may successively quadruple, quintuple—go to ten times the distance, the needle remains rigid in its adherence to the deflection of 60°. There is, to all appearance, no diminution whatever of intensity with the increase of distance.

From this experiment, which might at first sight appear fatal to the law of inverse squares, as applied to heat, Melloni, in the most ingenious manner, proved the law. I will here follow his reasoning. Imagine the hollow cone in front of the pile prolonged; it would cut the radiating surface in a circle, and this circle is the only

portion of that surface whose rays can reach the pile. All the other rays are cut off by the non-reflecting lining of the cone. When the pile is moved to double the distance, the section of the cone prolonged encloses a circle of double the diameter, or four times the area of the former one; at treble the distance, the radiating surface is augmented nine times; at ten times the distance, the radiating surface is augmented 100 times. Now, the constancy of the deflection proves that the augmentation of the surface must be exactly neutralised by the diminution of the intensity. But the radiating surface augments as the square of the distance, hence the intensity of the heat must diminish as the square of the distance; and thus the experiment, which might at first sight appear fatal to the law, demonstrates that law in the most simple and conclusive manner.

I spoke a moment ago of the dilution suffered by light through its diffusion over a large surface. This, however, is but a vague way of expressing the real fact. The diminution of intensity both of light and radiant heat is, in reality, a diminution of motion. Every ether particle, as a wave passes it, oscillates to and fro. At the two limits of its excursion it is brought momentarily to rest, midway between those limits its velocity is a maximum. *The intensity of the light is proportional to the square of this maximum velocity.* The range of the vibration of an ether particle is technically called its *amplitude*; and the intensity of the light is also proportional to the square of the amplitude. It can be proved that both the maximum velocity and the amplitude vary inversely as the simple distance from the radiant point; hence the intensity of the light and heat emitted by that point must vary inversely as the square of the distance.

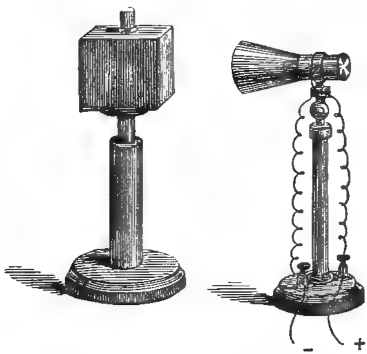
PHYSICAL MEANING OF RADIATION.

Let us now revert for a moment to our fundamental conceptions regarding radiant heat. Its origin is an oscillatory motion of the ultimate particles of matter—a motion taken up by the ether, and propagated through it in waves. There is an important difference between the motion of the ether particles and that of the air particles in the case of sound. The air particles move to and fro, in the direction in which the sound travels; the ether particles move to and fro, *across* the line in which the light travels. The vibrations of the air are longitudinal, those of the ether transversal. That this is the case has been inferred from optical phenomena. But it is manifest that the disturbance produced in the ether must depend upon the character of the oscillating body; one atom may be more unwieldy than another, and a single atom could not be expected to produce so great a disturbance as a group of atoms oscillating as a system. Thus, when different bodies are heated, we may fairly expect that their atoms, or molecules, will not all create the same amount of disturbance in the ether. It is probable that some will communicate a greater amount of motion than others: in other words, that some will radiate more copiously than others. For radiation, strictly defined, *is the communication of molecular motion from a heated body, to the ether in which it is immersed.*

Let us bring the conclusion here considered probable to the test of experiment. This cubical vessel C (fig. 94) is called a 'Leslie's cube,' because vessels of this shape were used by Sir John Leslie, in his researches on radiant heat. The vessel is of pewter, but one of its vertical sides is coated with a layer of gold, another with a layer of silver, a third with a layer of copper, while the fourth

is coated with a varnish of isinglass. Let us fill the cube with hot water, and, keeping it at a constant distance from the thermo-pile, P, allow its four faces to radiate, in succession, against the pile. The hot gold surface produces scarcely any deflection; the hot silver is equally inoperative; the same is the case with the copper; but when the *varnished* surface is turned towards the pile, the gush of heat becomes suddenly so great that the needle moves up to its stops. Hence we infer, that through some physical cause or other, the molecules of the varnish, when

FIG. 94.



agitated by heat, communicate more motion to the ether than do the atoms of the metals; in other words, the varnish is a better radiator than the metals are. A similar result is obtained when a silver teapot is compared with an earthenware one. Both being filled with boiling water, the silver produces but little effect, while the radiation from the earthenware is so copious, as to drive the needle up to 90°. If a pewter pot be compared with a glass beaker, when both are filled with hot water, the radiation from the glass proves to be much more powerful than that from the pewter.

You have often heard of the effect of colours on radiation,

and have doubtless heard a good deal that is unwarranted by experiment. Let me give a passing proof of this. One of the sides of this cube is coated with whiting, another with carmine, a third with lampblack, while the fourth is left uncoated. Filling the cube with boiling water, and presenting its black surface to the pile, the needle moves, and finally points steadily to 65°. The cube rests upon a little turn-table, by turning which the white face is presented to the pile. The needle remains stationary, proving the radiation from the white surface to be just as copious as that from the black. When the red surface is turned towards the pile, there is no change in the position of the needle. I now turn the uncoated side; the needle instantly falls towards 0°, proving the inferiority of the metallic surface as a radiator. I make the same experiments with another cube, the sides of which are covered with velvet; black, white, and red. The three velvet surfaces radiate alike, while the naked surface radiates less than any of them. These experiments show that the radiation from the clothes which cover the human body is not at all, to the extent sometimes supposed, dependent on their colour. The colour of an animal's fur is equally incompetent to influence the radiation. These are the conclusions arrived at by Leslie and Melloni *for obscure heat.* We shall subsequently push the investigation of this subject much beyond the point at which they left it.

Now if the coated surface in the foregoing experiments communicates more motion to the ether than the uncoated one, it necessarily follows that the coated vessel will cool more quickly than the uncoated one. We can test this immediately. Here are two metal vessels, one of which is covered with lampblack, while the other is bright. Three-quarters of an hour ago boiling water was poured into them, a thermometer being placed in each. Both thermometers then showed the same temperature,

but now one of them is some degrees below the other, the vessel which has cooled most rapidly being the coated one. Here, again, are two vessels, one of which is bright, and the other closely coated with flannel. Half an hour ago two thermometers, plunged in these vessels, showed the same temperature, but the covered vessel has now a temperature two or three degrees lower than the naked one. It is not unusual to preserve the heat of teapots by a woollen covering; but to be effective the 'cosy' must fit very loosely. A closely fitting cover which has the heat of the teapot freely imparted to it by contact, would, as we have seen, promote the loss which it is intended to diminish, and thus do more harm than good.

Rumford has been a source from which compilers have drawn freely in their expositions of the laws of radiant heat. His experiments, on which we have also drawn, are excellent, but his inferences are often defective. His notion of frigorific rays, for example, was a fundamental mistake, and his strong tendency to apply the results of science to practical life often led him to hasty conclusions. After proving that goldbeater's-skin, when blackened, was a better radiator, and consequently a more rapid cooler, than when left in its ordinary condition, he at once transfers the subject to the hands of the doctors. "I shall leave to physicians," he says, "what advantages may be derived from a knowledge of the facts they establish, in taking measures for the preservation of the health of Europeans who quit their native climate to inhabit hot countries. All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing could prevent me from making the experiment of blackening my skin, or at least of wearing a black shirt, in the shade, and especially at night; in order to find out if, by these means, I could not contrive to make myself more comfortable." He had a strong belief—at bottom a healthy one, but

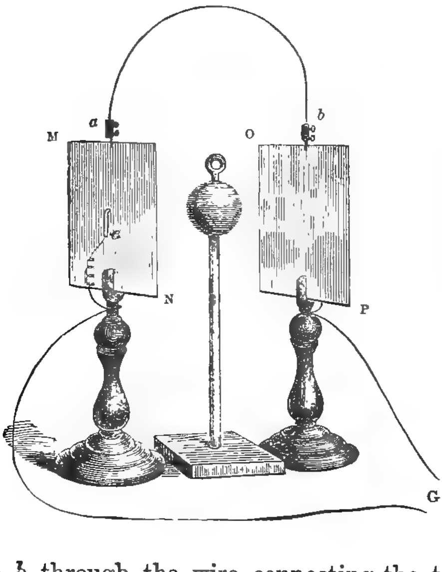
needing control—in the fitness of things, as established by experience. 'Several of the savage tribes,' he says, 'which inhabit very cold countries besmear their skins with oil, which gives them a shining appearance. The rays of light are reflected copiously from the surface of their bodies. May not the frigorific rays, which arrive at the surface of their skin, be also reflected by the highly polished surface of the oil with which it is covered? If that should be the case, instead of despising these poor creatures for their attachment to a useless and loathsome habit, we should be disposed to admire their ingenuity, or rather admire and adore the goodness of their invisible Guardian and Instructor, who teaches them to like, and to practise what may be useful to them.'

RECIPROCITY OF RADIATION AND ABSORPTION.

One of the most interesting points connected with our present subject is the reciprocity which exists between the power of a body to communicate motion to the ether, or to radiate; and its power to accept motion from the ether, or to absorb. As regards radiation, we have already compared lampblack and whiting with metallic surfaces; we will now compare the same substances, with reference to their powers of absorption. Of these two sheets of tin, M N, O P (fig. 95), one, O P, is coated with whiting, and the other, M N, left uncoated; I place them parallel to each other, and at a distance of about two feet asunder. To the upper edge of each sheet is soldered a screw, and between the two screws, from one sheet to the other passes a copper wire, *a*, *b*. At the back of each sheet is soldered one end of a little bar of bismuth, to the other end, *e*, of which, a wire is attached, terminated by a binding screw. With these two binding screws are connected the two ends of the wire, coming from the galvanometer beyond G.

You observe that we have now an unbroken metallic circuit, in which the galvanometer is included. Acquainted as you are with the thermo-pile, you doubtless know already what the bismuth bars are intended for. The bismuth and the tin in each case constitute a thermo-electric couple. When the warm finger is placed for a moment opposite to this left-hand junction, *e*, a current is developed which passes from the bismuth to the tin, thence

FIG. 95.



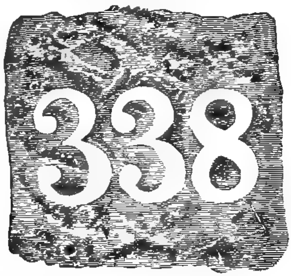
from *a* to *b* through the wire connecting the two sheets, thence through the tin to the other junction, thence round the galvanometer, and back to the point from which it started. The needle moves through a large arc; the red end going towards you. Placing my finger against the other junction, a deflection in the opposite direction is the consequence. When the finger is withdrawn, the junctions cool, and the needle sinks to zero.

Exactly midway between the two sheets of tin, is set a stand on which is placed a heated copper ball, which radiates against both sheets. On the right, however, the rays strike upon a coated surface, while on the left they strike upon a naked metallic one. If both surfaces absorbed equally the radiant heat—if both accepted with equal freedom the motion of the ethereal waves—the bismuth junctions at the backs would be equally warmed, and one of them would neutralise the other. But if one surface absorb more freely than the other, a deflection of the galvanometer needle will occur, the direction of which will tell us which is the best absorber. The ball is now upon the stand, and the prompt and energetic motion of the needle informs us that the coated surface is most heated. In the same way I compare lampblack and varnish with tin, and find the two former to be by far the best absorbers.

The thinnest metallic film furnishes a powerful defence against the absorption of radiant heat. Coating the back of a sheet of 'gold paper'—the gold being merely copper reduced to great tenuity—with the red iodide of mercury, I lay the paper flat on a board, with the coloured surface downwards. On its upper metallic surface are pasted pieces of paper so as to form a complicated pattern. I now pass a red-hot spatula several times over the sheet without touching it; the spatula radiates strongly against the sheet, but its rays are absorbed in very different degrees. The metallic surface absorbs but little, and is but little heated; the paper surfaces absorb greedily and become very hot. On turning up the sheet, you see that the iodide underneath the metallic portion is perfectly unchanged, while under every bit of paper the colour is discharged. This is a well-known effect of heat on the iodide of mercury. An exact copy of the figures pasted on the upper surface of the sheet is thus formed. For another

example of the same kind, I am indebted to Mr. Hyde Hills. The radiant heat of a fire impinged against this painted piece of wood (fig. 96), on which the number 338 was printed in gold-leaf letters; the paint is blistered and charred all round the letters, but underneath the letters both wood and paint are quite unaffected. This thin film

Fig. 96.



of gold proved quite sufficient to prevent the absorption, to which the destruction of the surrounding surface is due.

DIATHERMANCY. MELLONI'S RESEARCHES.

The luminiferous ether fills stellar space; it makes the universe a whole, and renders possible the intercommunication of light and energy between star and star. But the subtle substance penetrates farther; it surrounds the very atoms of solid and liquid substances. Transparent bodies are those which are so related to the ether that the waves of light can pass through them without transference of motion to their atoms. In coloured bodies, certain waves are absorbed; but those which give the body its colour pass without absorption. Through a solution of sulphate of copper, for example, the blue waves speed unimpeded, while the red waves are destroyed. When a luminous

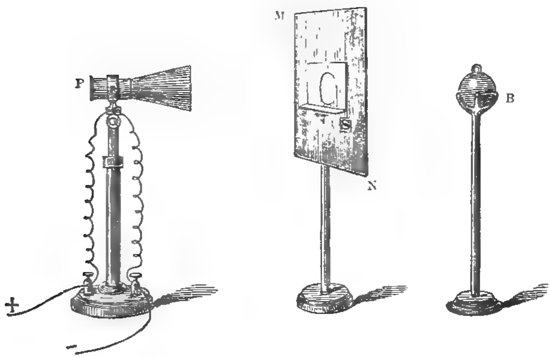
beam is sent through this solution, the red end of its spectrum is cut away. Red glass, on the contrary, owes its colour to the fact that its substance can be traversed freely by the longer undulations of red, while the shorter waves are absorbed. Placed in the path of the light, it leaves merely a vivid red band upon the screen. The blue liquid, then, cuts off the rays transmitted by the red glass; and the red glass cuts off those transmitted by the liquid; by the union of both we ought to have perfect opacity, and so we have. When both are placed in the path of the beam, the entire spectrum disappears; the union of the two partially transparent bodies producing an opacity, equal to that of pitch or metal.

A solution of the permanganate of potash placed in the path of the beam permits the two ends of the spectrum to pass freely through; we have the red and the blue, but between both a space of intense blackness. The yellow of the spectrum is pitilessly destroyed by this liquid. Hence its gorgeous colour. Projecting a disk of white light two feet in diameter upon the screen, I place a flat cell containing the permanganate in the path of the beam. Nothing could be more splendid than the purple of that disk. Turning the lamp obliquely, and introducing a prism, the blue component of the colour slides away from the red. You see two disks which overlap in the centre, and exhibit there the tint of the composite light which passes through the liquid.

Thus, as regards the waves of light, bodies exercise, as it were, an elective power, singling out certain waves for destruction, and permitting others to pass. Transparency to waves of one length does not imply transparency to waves of another length, and from this we might reasonably infer, that transparency to light does not necessarily imply transparency to radiant heat. This conclusion can be verified by experiment. To do so a tin screen, M N

(fig. 97), is pierced by an aperture, behind which is soldered a small stand S. A copper ball, B, heated to dull redness, is placed on a proper stand, on one side of the screen. On the other side is placed the thermo-pile, P; the rays from the ball now pass through the aperture and fall upon the pile. The needle moves, and finally comes to rest with a steady deflection of 80°. I place a glass

FIG. 97.



cell, a quarter of an inch wide, filled with distilled water, on the stand S, so that all rays reaching the pile must pass through the water. What takes place? The needle steadily sinks to zero; scarcely a ray from the hot ball can cross the water; though so extremely transparent to light, to the undulations issuing from the ball the water is practically opaque. Before removing the cell of water, I place behind it a similar cell, containing transparent bisulphide of carbon; so that when the water-cell is removed, the aperture shall still be barred by the new liquid. What occurs? The needle promptly moves upwards, and describes a large arc; so that the selfsame rays which found the water impenetrable, find easy access through the bisulphide of carbon. In the

same way, when alcohol is compared with chloride of phosphorus, we find the former almost opaque to the rays emitted by our warm ball, while the latter permits them to pass freely.

Similar differences are observed among solid bodies. A plate of very pure glass is now placed on the stand s, and for the hot ball we substitute a cube containing hot water. No movement of the needle is perceptible. Displacing the plate of glass by a plate of rock-salt of much greater thickness, the needle promptly moves, until arrested by its stops. To these rays, then, rock-salt is eminently transparent, while glass is practically opaque to them.

For these, and numberless similar results, we are indebted to Melloni, who enormously augmented our knowledge of the transmission of radiant heat through solids and liquids. To express this power of transmission, he proposed the word *diathermancy*. Diathermancy then bears the same relation to radiant heat that transparency does to light. Instead of giving you, at this stage of our inquiries, determinations of my own of the diathermancy of solids and liquids, I will make a selection from the tables of the eminent Italian philosopher just referred to. In these determinations, Melloni used four different sources of heat: the flame of a Locatelli lamp; a spiral of platinum wire, kept incandescent by the flame of an alcohol lamp; a plate of copper heated to 400° Cent., and a plate of copper heated to 100° Cent., the last-mentioned source being the surface of a copper cube, containing boiling water. The experiments were made in the following manner:—First, the radiation of the source, that is to say the galvanometric deflection produced by it, was determined, when nothing but air intervened between the source of heat and the pile. This deflection expressed the total radiation. Then the substance whose diathermancy

was to be examined was introduced, and the consequent deflection noted; this deflection expressed the quantity of heat transmitted by the substance. Calling the total radiation 100, the proportionate quantities transmitted by twenty-five different substances are given in the annexed table.

Names of Substances—reduced to a common thickness of $\frac{1}{10}$th of an inch (2·6 millims)	Transmissions: percentage of the total Radiation			
	Locatelli Lamp	Incandescent Platinum	Copper at 400° C.	Copper at 100° C.
1 Rock-salt	92·3	92·3	92·3	92·3
2 Sicilian sulphur	74	77	60	54
3 Fluor spar	72	69	42	33
4 Beryl	54	23	13	0
5 Iceland spar	39	28	6	0
6 Glass	39	24	6	0
7 Rock-crystal (clear)	38	28	6	3
8 Smoky quartz	37	28	6	3
9 Chromate of potash	34	28	15	0
10 White topaz	33	24	4	0
11 Carbonate of lead	32	23	4	0
12 Sulphate of baryta	24	18	3	0
13 Felspar	23	19	6	0
14 Amethyst (violet)	21	9	2	0
15 Artificial amber	21	5	0	0
16 Borate of soda	18	12	8	0
17 Tourmaline (deep green)	18	16	3	0
18 Common gum	18	3	0	0
19 Selenite	14	5	0	0
20 Citric acid	11	2	0	0
21 Tartrate of potash	11	3	0	0
22 Natural amber	11	5	0	0
23 Alum	9	2	0	0
24 Sugar-candy	8	1	0	0
25 Ice	6	0·5	0	0

This table shows, in the first place, what very different transmissive powers different solid bodies possess. It shows us also that, with a single exception, the diathermancy of the bodies mentioned varies with the source of the heat. Rock-salt, only, is equally transparent to heat from the four sources. It must here be borne in

mind that the light-rays are also heat-rays; that the selfsame ray, falling upon the nerve of vision, produces the impression of light; while, impinging upon other nerves of the body, it produces the impression of heat. The light-rays have, however, a shorter wave-length than the obscure heat-rays: and knowing, as we do, how differently waves of different lengths and periods are absorbed by bodies, we are in a measure prepared for the results of the foregoing table. Thus while glass of the thickness here specified permits 39 per cent. of the rays of Locatelli's lamp and 24 per cent. of the rays from the incandescent platinum to pass, it transmits only 6 per cent. of the rays from a source of 400° C., while it is absolutely opaque to all rays emitted from a source of 100° C. We also see that limpid ice, so highly transparent to light, transmits only 6 per cent. of the rays of the lamp, and 0·5 per cent. of the rays of the incandescent platinum, while it cuts off all rays issuing from the other two sources. We have here an intimation, that by far the greater portion of the rays emitted by the lamp of Locatelli must be obscure. Luminous rays pass through ice, of the thickness here given, without sensible absorption, and the fact that 94 per cent. of the rays issuing from Locatelli's flame are destroyed by the ice, proves that this proportion of these rays has no light-giving power. As regards the influence of transparency, clear and smoky quartz are very instructive. Here are two substances, one perfectly pellucid, the other a dark brown; still, for the luminous rays only do these two specimens show a difference of transmission. The clear quartz transmits 38 per cent., and the smoky quartz 37 per cent. of the rays from the lamp, while, for the other three sources, the transmissions of both substances are identical.

Melloni supposed rock-salt to be perfectly transparent to all kinds of calorific rays, the 7·7 per cent. less than a

hundred which the foregoing table exhibits, being due, not to absorption, but to reflection at the two surfaces of the plate of salt. But the accurate experiments of MM. de la Provostaye and Desains prove that this substance is permeable in different degrees to heat of different kinds; while Mr. Balfour Stewart has shown that rock-salt is particularly opaque to rays issuing from a heated piece of the same substance. We shall return to this important subject.

In the following table, which is also taken from Melloni, the transmissions of nineteen different liquids are given. The source of heat was an Argand lamp, furnished with a glass chimney, and the liquids were enclosed in a cell with glass sides, the thickness of the liquid layer being 9·21 millimetres, or 0·36 of an inch. Liquids are here shown to be as diverse in their powers of transmission as solids: and it is also worthy of remark, that water maintains its position as regards opacity, notwithstanding the change in its state of aggregation.

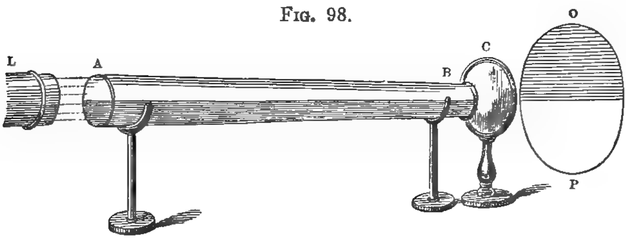
Names of Liquids; thickness, 0·36 in.	Transmission: percentage of total radiation
1 Bisulphide of carbon	63
2 Bichloride of sulphur	63
3 Protochloride of phosphorus	62
4 Essence of turpentine	31
5 Olive oil	30
6 Naphtha	28
7 Essence of lavender	26
8 Sulphuric ether	21
9 Sulphuric acid	17
10 Hydrate of ammonia	15
11 Nitric acid	15
12 Absolute alcohol	15
13 Hydrate of potash	13
14 Acetic acid	12
15 Pyroligneous acid	12
16 Concentrated solution of sugar	12
17 Solution of rock-salt	12
18 White of egg	11
19 Distilled water	11

The reciprocity, which we have already demonstrated between radiation and absorption, in the case of metals, varnishes, &c., may now be extended to the bodies contained in Melloni's tables. One or two illustrations, taken from an extremely suggestive memoir by Mr. Balfour Stewart, will be sufficient. In this copper vessel water is kept in a state of gentle ebullition. On the flat copper lid of the vessel are laid plates of glass and of rock-salt, until they assume the temperature of the lid. When the plate of heated rock-salt is fixed upon a stand, in front of the thermo-electric pile, the deflection produced is so small as to be scarcely sensible. When a plate of heated glass is fixed upon the stand, the needle moves through a large arc, thus conclusively showing that the glass, which is the more powerful absorber of obscure heat, is also the more powerful radiator. Alum, unfortunately, melts at a temperature lower than that here made use of. When close to its point of fusion, its temperature is not so high as that of the glass, still you can see that it transcends the glass as a radiator.

Absorption takes place *within* the absorbing body, a certain thickness being requisite to effect the absorption. This is true of both light and radiant heat. A very thin stratum of pale ale is almost as colourless as a stratum of water, the absorption being too inconsiderable to produce the decided tint which larger masses of the liquid exhibit. When distilled water is poured into a drinking glass, it exhibits no trace of colour; but an experiment is here arranged which will show you that this pellucid liquid, in sufficient thickness, has a very decided colour. This tube A B (fig. 98), fifteen feet long, is placed horizontally, its ends being stopped by pieces of plate glass. At one end of the tube stands an electric lamp, L, from which a cylinder of light will be sent through the tube.

It is now half filled with water, the upper surface of which cuts the tube into two equal parts horizontally. Thus, half of the beam will pass through air, and half through water; while with a lens, C, a magnified image of the adjacent end of the tube is projected on the screen. You see the image, O P, composed of two semicircles, one formed by the light which has passed through the water, the other by the light which has passed through the air. Placed thus, side by side, you can accurately compare them, and you notice that while the air semicircle is a

FIG. 98.



pure white, the water semicircle is a bright and delicate blue-green. Thus, by augmenting the thickness through which the light has to pass, we deepen the colour; proving thereby that the destruction of the light occurs within the absorbing body, and that it is not an effect of surface merely.

Melloni showed the same to be true of radiant heat. In his experiments, already recorded at page 308, the thickness of the plates used was 2·6 millimetres, but by rendering the plate thinner, he enabled a greater quantity of heat to get through it. The following table shows the influence of thickness on the transmissive power of a plate of glass.

Thickness of Plates in millimetres	Transmissions by Glass of different thicknesses: percentage of the total Radiation			
	Locatelli Lamp	Incandescent Platinum	Copper at 400° C.	Copper at 100° C.
2·6	39	24	6	0
0·5	54	37	12	1
0·07	77	57	34	12

Thus, we see that by diminishing the thickness of the plate from 2·6 to 0·07 millimetres, the quantity of heat transmitted rises, in the case of the lamp of Locatelli, from 39 to 77 per cent.; in the case of the incandescent platinum, from 24 to 57 per cent.; in the case of copper at 400° C., from 6 to 34 per cent.; and in the case of copper at 100° C., from absolute opacity to a transmission of 12 per cent.

The influence of the thickness of a plate of selenite on the quantity of heat which it transmits, is exhibited in the following table:—

Thickness of Plates in millimetres	Transmissions by Selenite of different thicknesses: percentage of the total Radiation			
	Locatelli Lamp	Incandescent Platinum	Copper at 400° C.	Copper at 100° C.
2·6	14	5	0	0
0·4	38	18	7	0
0·01	64	51	32	21

These experiments prove conclusively that the absorption of heat takes place within the body, and is not a surface action.

To reach its maximum, radiation must also come from a certain depth. Beginning, for instance, with a thin layer of varnish on a metallic surface, Rumford found the radiation augmented by the application of a second and a

third layer. The varnish, therefore, is, to some extent, transparent to its own radiation.

SIFTING OF RADIANT HEAT.

The decomposition of the solar beam produces the solar spectrum; luminous in the centre, thermal at one end, and chemical at the other. The sun is, therefore, a source of heterogeneous rays, and there cannot be a doubt that all ordinary sources of heat, luminous and obscure, partake of this heterogeneity. In general, when such mixed rays enter a diathermic substance, some are intercepted, others permitted to pass. Supposing, then, that we take a sheaf of calorific rays, which have already passed through a diathermic plate, and permit them to fall upon a second plate of the same material, the transparency of this second plate to the heat incident upon it, must be greater than the transparency of the first plate to the heat incident on *it*. The first plate, if sufficiently thick, has already extinguished, in great part, the rays which the substance is capable of absorbing; and the residual rays, as a matter of course, pass freely through a second plate of the same substance. The original beam is *sifted* by the first plate, and the purified beam possesses, for the same substance, a higher penetrative power than the original beam.

This power of penetration has usually been taken as a test of the *quality* of heat; the heat of the purified beam is said to be different in quality from that of the unpurified beam. It is not, however, that any individual ray or wave has changed its character, but that from the beam, as a whole, certain constituents have been withdrawn; and that this withdrawal has altered the proportion of the incident heat transmitted by a second substance This is the true meaning of the term 'quality,' as here applied to radiant heat. In the path of the beam from a

lamp, let plates of rock-salt, alum, bichromate of potash, and selenite be successively placed, each plate 2·6 millimetres in thickness: let the heat emergent from each plate fall upon a second plate of the same substance; out of every hundred units of this heat, the following proportions are, according to Melloni, transmitted:—

Rock-salt . . .	92·3
Alum	90
Chromate of potash. .	71
Selenite	91

Referring to the table, p. 308, we find, that of the whole heat emitted by the Locatelli lamp, only 34 per cent. is transmitted by the chromate of potash: here we find the percentage 71. Of the entire radiation, selenite transmits only 14 per cent., but of the beam which has been purified by a plate of its own substance, it transmits 91 per cent. The same remark applies to the alum, which transmits only 9 per cent. of the unpurified beam, and 90 per cent. of the purified beam. In rock-salt, on the contrary, the transmissions of the sifted and unsifted beam are the same, because the substance is equally transparent to rays of all the qualities here employed. In these cases, I have supposed the beam emergent from rock-salt to pass though rock-salt; the beam emergent from alum to pass through alum, and so of the others; but, as might be expected, the sifting of the beam by any substance, will alter the proportion in which it will be transmitted by almost any other second substance.

I will conclude these observations with an experiment which will show you the influence of sifting, in a very striking manner. Here is an air-thermometer with a clean glass bulb, and so sensitive that the slightest touch of the hand causes a depression of the thermometric column. Converging the rays from our electric lamp on the bulb of that thermometer, the air within it is traversed

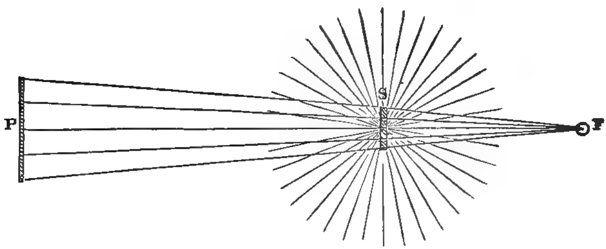
by a beam of intense power; but not the slightest depression of the thermometric column is discernible. When this experiment was first shown to a person here present, he almost doubted the evidence of his senses; but the explanation is simple. The beam, before it reaches the bulb, is already sifted by the glass lens used to concentrate it; and having passed through 12 or 14 feet of air, it contains no constituent, which can be sensibly absorbed by the air within the bulb. Hence, the purified beam passes through both air and glass, without warming either. It is competent, however, to warm the thermo-pile, whose exposure to it, for a single instant, drives the galvanometer needle violently aside. Covering, moreover, with lampblack the portion of the glass bulb struck by the beam, the heat is absorbed, the air expands, and the thermometric column is forcibly depressed.

We use glass fire-screens, which allow the pleasant light of the fire to pass, while they cut off a large portion of the heat; the reason is, that by far the greater part of the heat emitted by a fire is obscure, and to this the glass is opaque. But in no case is there any loss. The heat absorbed by the glass warms it; the motion of the ethereal waves is here transferred to the molecules of the solid body. But you may be inclined to urge, that, under these circumstances, the glass itself ought to become a source of heat, and that, therefore, we ought to derive no benefit from the absorption. The fact is so, but the conclusion is unwarranted. The philosophy of the screen is this:—Let F (fig. 99) be a point of a fire, from which the rays proceed in straight lines, towards a person at P. Before the screen is introduced, each ray pursues its course direct to P; but now let a screen be placed at S. The screen intercepts the heat, and becomes warmed; but instead of sending on the rays in their original direction only, it, as a warm body, emits them *in all directions*. Hence, it

cannot transmit to the person at P all the heat intercepted. A portion of the heat is restored, but by far the greater part is diverted from P, and distributed in other directions.

Where the waves pursue their way unabsorbed, no motion of heat is imparted, as we have seen in the case of the air thermometer. A joint of meat might be roasted before a fire, with the air around the joint as cold as ice. The air on high mountains may be intensely cold, while a burning sun is overhead; the solar rays which, striking on

Fig. 99.



the human skin, are almost intolerable, are incompetent to heat the air sensibly, and we have only to withdraw into perfect shade, to feel the chill of the atmosphere. I never, on any occasion, suffered so much from solar heat, as in descending from the 'Corridor' to the Grand Plateau of Mont Blanc, on August 13, 1857. Though Mr. Hirst and myself were at the time hip deep in snow, the sun blazed against us with unendurable power. Immersion in the shadow of the Dôme du Goûté at once changed our feelings; for here the air was at a freezing temperature. It was not, however, sensibly colder than the air through which the sunbeams passed; and we suffered, not from the contact of hot air, but from radiant heat, which had reached us through an icy cold medium.

The beams of the sun penetrate glass without sensibly

heating it; the reason is, that having passed through our atmosphere, the heat has been in a great measure deprived of those constituents liable to be absorbed by glass. An experiment was made on a former occasion, which you will now completely understand. A beam was sent from the electric lamp through a plate of ice, without melting it. The beam had been previously sifted by sending it through a vessel of water, in which the heat capable of being absorbed by the ice was lodged, and lodged so copiously, that the water was raised almost to the boiling point during the experiment. It is here worthy of remark, that the liquid water and the solid ice appear to be pervious and impervious to the same rays; the one may be used as a *sieve* for the other: a result which indicates that the absorption is not influenced, in this case, by the difference of aggregation. It is easy to prove that the beam which has traversed ice without melting it, is really a calorific beam. Allowing it to fall upon our thermo-electric pile, it causes the needle to move with energy to its stops.

When the calorific waves are intercepted, they usually raise the temperature of the body by which they are absorbed; but when the absorbing body is ice, at a temperature of 32° Fahr., it is impossible to raise its temperature. How, then, does the heat absorbed by the ice employ itself? It produces internal liquefaction, taking down the crystalline atoms, and forming those beautiful liquid flowers shown to you on a former occasion.

INVISIBLE RADIATION.

We have seen that transparency is not at all a test of diathermancy; that a body, highly transparent to the luminous undulations, may be highly opaque to the non-luminous ones. A body may, as we have already seen, be

absolutely opaque to light, and still, in a considerable degree, transparent to heat. The convergent beam of the electric lamp now marks its course through the dust of the room: you see the point of convergence of the beam, at a distance of fifteen feet from the lamp. Let us mark that point accurately. This plate of rock-salt is coated so thickly with the soot of a smoky flame that the light, not only of every gas lamp in this room, but the electric light itself, is cut off by it. Placing this plate of smoked salt in the path of the beam, the light is intercepted, but the mark enables me to find the place where the focus fell. I place the pile at this point: you see no beam falling on it, but the violent action of the needle instantly reveals to the mind's eye a focus of invisible heat.

You might, perhaps, be disposed to think that the heat falling on the pile has been first absorbed by the soot, and then radiated from it, as from an independent source. Melloni has removed every objection of this kind; but not one of his experiments is, I think, more conclusive, as a refutation of the objection, than that now performed before you. For if the smoked salt were the source, the rays could not converge here to a focus, the salt being *at this side* of the converging lens. You also see, that when the pile is displaced a little, laterally, but still turned towards the smoked salt, the needle sinks to zero. The heat, moreover, falling on the pile, is, as shown by Melloni, practically independent of the position of the plate of rock-salt; you may cut off the beam, at a distance of fifteen feet from the pile, or at a distance of one foot: the result is sensibly the same, which could not be the case, if the smoked salt itself were the source of the radiation.

When the experiment is repeated with black glass, the result is the same. Now, the glass reflects a considerable portion of the light and heat from the lamp;

when it is held a little obliquely to the beam you can see the reflected light. While the glass is in this position, I will coat it with a layer of lampblack, which, thus applied, is a powerful absorber. Though the glass plate now stops the heat which a moment ago passed through it, it has ceased to affect the pile, and the needle descends to zero, thus furnishing additional proof that the heat which, in the first place, acted upon the pile, came not from the glass but from the lamp, crossing the opaque substance as light traverses a transparent one.

Rock-salt, according to Melloni, transmits all rays luminous and obscure; alum, of the thickness already given, transmits only the luminous rays;[1] hence, the difference between alum and rock-salt ought to give the value of the obscure radiation. Tested in this way, Melloni found the following proportions of luminous to obscure rays, for the following three sources:—

Source	Luminous	Obscure
Flame of oil	10	90
Incandescent platinum . .	2	98
Flame of alcohol . . .	1	99

Thus, of the heat radiated from the flame of oil, 90 per cent.; of the heat radiated from incandescent platinum, 98 per cent.; while of the heat radiated from the flame of alcohol, fully 99 per cent. is due to the obscure emission.

[1] We shall subsequently learn that this is an error.

LECTURE XII.

ABSORPTION OF HEAT BY GASEOUS MATTER—APPARATUS EMPLOYED—EARLY DIFFICULTIES — DIATHERMANCY OF AIR AND OF THE TRANSPARENT ELEMENTARY GASES—ATHERMANCY OF OLEFIANT GAS AND OF OTHER COMPOUND GASES — ABSORPTION OF RADIANT HEAT BY VAPOURS — RADIATION OF HEAT BY GASES—RECIPROCITY OF RADIATION AND ABSORPTION —INFLUENCE OF MOLECULAR CONSTITUTION ON THE PASSAGE OF RADIANT HEAT - TRANSMISSION OF HEAT THROUGH OPAQUE BODIES— HEAT-SPECTRUM DETACHED FROM LIGHT-SPECTRUM BY AN OPAQUE PRISM —RADIATION THROUGH AIR.

RADIATION THROUGH AIR.

WE have now examined the diathermancy, or transparency to heat, of solid and liquid bodies; and have learned that, closely as the atoms of such bodies are packed together, the interstitial spaces between the atoms offer in many cases a free passage to the ethereal undulations. In other cases, however, we found that the atoms stopped the waves of heat which impinged upon them; but that in so doing, they themselves became centres of heat-motion. Prior to the spring of 1859 our knowledge was limited to the action of solid and liquid bodies on radiant heat, no single successful experiment having been previously made on matter in the gaseous state of aggregation. The domain opened up for the first time in 1859 we enter to-day, and in it for the future our inquiries are, in great part, to be conducted.

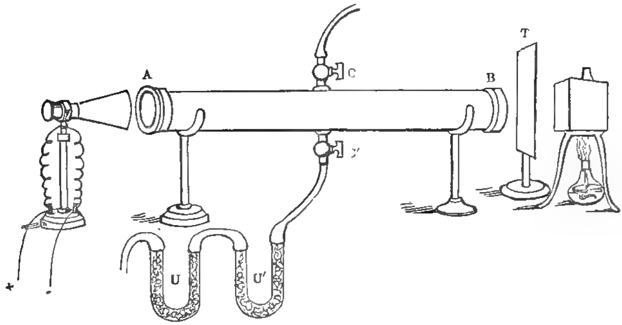
In gaseous bodies the interatomic spaces are so vastly augmented, compared with those of liquids and solids—the molecules, moreover, are so completely released from all mutual entanglement, that scientific men were almost justified in concluding *à priori* that gases and vapours would furnish a perfectly open door for the passage of the heat-waves. This conclusion seemed verified by such experiments as had been made on atmospheric air, which was found by our ablest experimenters, including Melloni himself, to give no evidence of absorption.

But each succeeding year augments our experimental power, the invention of improved methods enabling us to renew our inquiries with increased chances of success. Let us, then, test again with more delicate apparatus the diathermancy of atmospheric air. We make a preliminary essay with a hollow tin cylinder (A B, fig. 100), 4 feet long, and nearly 3 inches in diameter. Our object being to compare the passage of radiant heat through the air with its passage through a vacuum, we must have some means of stopping the ends of our cylinder, so as to be able to exhaust it. Here we encounter our first experimental difficulty. In the case of air, non-luminous heat is more likely to be absorbed than luminous heat, and as our object is to make the absorption of air sensible, we augment our chance of effecting this object by employing the radiation from an obscure source.

Our tube, therefore, must be stopped by a substance which permits of the free passage of invisible heat. Shall we use glass for the purpose? An inspection of the table at page 308 shows us, that for such heat, plates of glass would be as perfectly opaque as plates of metal. Observe here how one investigator's results are turned to account by another: how science grows by the continual subjugation of ends to means. Had not Melloni discovered the diathermic properties of rock-salt, we should now be

utterly at a loss. For a time, however, the difficulty of obtaining plates of salt sufficiently large and pure to stop the ends of my tube was very great. But a scientific worker, if his wants are made known, does not long lack help; and, thanks to such friendly aid, I have here trans-

FIG. 100.



parent plates of this precious substance, which, by means of the caps A and B, can be screwed air-tight on to the ends of the cylinder.[1] The cylinder is provided with two

[1] At a time when I was greatly in need of a supply of rock-salt, I stated my wants in the 'Philosophical Magazine,' and met with an immediate response from Sir John Herschel. He sent me a block of salt, accompanied by a note, from which, as it refers to the purpose for which the salt was originally designed, I will make an extract. I am also greatly indebted to Dr. Szabo, the Hungarian Commissioner to the International Exhibition of 1862, by whom I have been raised to comparative opulence, as regards the possession of rock-salt. To the Messrs. Fletcher of Northwich, and to Mr. Corbett of Bromsgrove, my best thanks are also due for their ready kindness.

To these acknowledgments I have now to add my respectful thanks to the government of Würtemburg, for the noble block of salt placed in their department in the Paris Exhibition of 1867.

Here follows the extract from Sir J. Herschel's note :—'After the publication of my paper in the Phil. Trans. 1840, I was very desirous to disengage myself from the influence of glass prisms and lenses, and ascertain, if possible, whether in reality my insulated heat spots β γ δ ϵ in the spectrum were of solar or terrestrial origin. Rock-salt was the obvious resource, and after many and fruitless endeavours to obtain sufficiently large and pure specimens, the late Dr. Somerville was so good as to send me (as I un-

stopcocks, one of which, C, is connected with an air-pump, by which the tube can be exhausted; while through the other cock, C′, air, or any other gas, can be allowed to enter the tube.

At one end of the cylinder is placed a Leslie's cube C, containing boiling water, and coated with lamp-black, to augment its radiation. At the other end stands our thermo-electric pile, from which wires lead to a galvanometer. Between the end B of the cylinder and the cube C, is introduced a tin screen, T, which, when withdrawn, will allow the calorific rays to pass from C through the tube. We first exhaust the cylinder, then draw the screen a little aside, and now permit the rays after traversing the vacuum to fall upon the pile. The tin screen, you observe, is only partially withdrawn, and the steady deflection, produced by the heat at present transmitted, is 30 degrees.

Let us now admit dry air; we do so by means of the cock C′, from which a piece of flexible tubing leads to the bent tubes U, U′, the first filled with fragments of pumice stone moistened with a solution of caustic potash, and intended to intercept whatever carbonic acid may be contained in the air. The tube U′ is filled with

derstood from a friend in Cheshire) the very fine block which I now forward. It is, however, much cracked, but I have no doubt pieces large enough for lenses and prisms (especially if cemented together) might be got from it.

'But I was not prepared for the working of it—evidently a very delicate and difficult process (I proposed to *dissolve* off the corners, &c., and, as it were, *lick* it into shape), and though I have never quite lost sight of the matter, I have not yet been able to do anything with it; meanwhile, I put it by. On looking at it a year or two after, I was dismayed to find it had lost much by deliquescence. Accordingly, I potted it up in salt in an earthen dish, with iron rim, and placed it on an upper shelf in a room with an Arnott stove, where it has remained ever since.

'If you should find it of any use, I would ask you, if possible, to repeat my experiment as described, and settle that point, which has always struck me as a very important one.'

fragments of pumice stone moistened with sulphuric acid, which is intended to absorb the aqueous vapour of the air. Thus, the air will reach the cylinder deprived both of its aqueous vapour and its carbonic acid. As the air enters, the mercury gauge of the pump will descend until the tube is quite full. If then air be a substance competent to intercept the waves of ether in any sensible degree, the withdrawal of the heat will be declared by the diminished deflection of the galvanometer. You, however, see no change in the position of the needle, nor could you see any change, even if you were close to the instrument. The air thus examined seems as transparent to radiant heat as the vacuum itself.

By gradually withdrawing the screen we can alter the amount of heat falling upon the pile; causing the needle to stand at 40°, 50°, 60°, 70°, and 80°, in succession. While it occupies each position, the test just applied can be repeated. In no instance could you recognise the slightest movement of the needle. The same is found to be the case when the screen is pushed forward, so as to reduce the deflection to 20 or 10 degrees.

The experiment just made is a question addressed to Nature, and her silence might be construed into a negative reply. But the experimental investigator must not lightly accept a negative, and I am not sure that we have put our question in the best possible way. Let us analyse what we have done; and first consider the case of our smallest deflection of 10 degrees. Supposing that the air is *not* perfectly diathermic; that it really intercepts a small portion—say the thousandth part of the heat passing through the tube—should we be able to detect this action? Such absorption, if it took place, would lower the deflection the thousandth part of ten degrees, or the hundredth part of one degree, a diminution which it would be impossible for you to see, even if you were close to the

galvanometer.[1] In the case here supposed, *the total quantity of heat falling upon the pile is so inconsiderable, that a small fraction of it, even if absorbed, might well escape detection.*

But we have not confined ourselves to a small quantity of heat; the result was the same when the deflection was 80° as when it was 10°. Here I must ask you to sharpen your attention and accompany me, for a short time, over rather difficult ground. I want now to make clearly intelligible to you an important peculiarity of the galvanometer.

The needle being at zero, let us suppose a quantity of heat to fall upon the pile, sufficient to produce a deflection of one degree. Suppose the quantity of heat to be afterwards augmented, so as to produce deflections of two degrees, three degrees, four degrees, five degrees; then the quantities of heat which produce these deflections stand to each other in the ratios of 1 : 2 : 3 : 4 : 5 : the quantity of heat which produces a deflection of 5° being exactly five times that which produces a deflection of 1°. But this proportionality exists only so long as the deflections do not exceed a certain magnitude. For, as the needle is drawn more and more aside from zero, the current acts upon it at an ever augmenting disadvantage. The case is illustrated by a sailor working a capstan; he always applies his strength at right angles to the lever, for, if he applied it obliquely, only a portion of that strength would be effective in turning the capstan round. And in the case of our electric current, when the needle is very oblique to the current's direction, only a portion of its force is effective in turning the needle. Thus it happens, that though the quantity of heat may be, and, in our case, *is*, accurately expressed by the strength of the current which it excites,

[1] It will be borne in mind that I am here speaking of *galvanometric*, not of *thermometric* degrees.

still the larger deflections, inasmuch as they do not give us the action of the whole current, but only of a part of it, cannot be a true measure of the amount of heat falling upon the pile.

The galvanometer now before you is so constructed, that the angles of deflection, up to 30° or thereabouts, are proportional to the quantities of heat; the quantity necessary to move the needle from 29° to 30° being sensibly the same as that required to move it from 0° to 1°. But beyond 30° the proportionality ceases. The quantity of heat required to move the needle from 40° to 41° is three times that necessary to move it from 0° to 1°; to deflect it from 50° to 51° requires five times the heat necessary to move it from 0° to 1°: to deflect it from 60° to 61° requires about seven times the heat necessary to move it from 0° to 1°; to deflect it from 70° to 71° requires eleven times, while to move it from 80° to 81° requires more than fifty times the heat necessary to move it from 0° to 1°. Thus, the higher we go, the greater is the quantity of heat represented by a degree of deflection; the reason being, that the force which then moves the needle is only a fraction of the force of the current really circulating in the wire, and hence represents only a fraction of the heat falling upon the pile.

By a process, to be afterwards described,[1] the higher degrees of the galvanometer can be expressed in terms of the lower ones. We thus learn, that while deflections of 10°, 20°, 30°, respectively, express quantities of heat represented by the numbers 10, 20, 30, a deflection of 40° represents a quantity of heat expressed by the number 47; a deflection of 50° expresses a quantity of heat expressed by the number 80; while the deflections 60°, 70°, 80°, express quantities of heat which increase in a much more rapid ratio than the deflections themselves.

[1] See Appendix to this Lecture.

What is the upshot of this analysis? It will lead us to a better method of questioning Nature. It suggests the reflection that, when we make our angles *small*, the quantity of heat falling on the pile is so inconsiderable, that even if a fraction of it were absorbed, it might escape detection; while, if we make our deflections large, by employing a powerful flux of heat, the needle is in a position from which it would require a considerable addition or subtraction of heat to move it. The 1,000th part of the whole radiation, in the one case, would be too small, absolutely, to be measured: the 1,000th part in the other case might be considerable, without, however, being considerable enough to affect the needle in any sensible degree. When, for example, the deflection is over 80°, an augmentation or diminution of heat, equivalent to 15 or 20 of the lower degrees of the galvanometer, would be scarcely sensible.

We are now face to face with our problem: it is this, to work with a flux of heat so large that a small fractional part of it will not be infinitesimal, and still to keep our needle in its most sensitive position. If we can accomplish this, we shall augment indefinitely our experimental power. If a fraction of the heat, however small, be intercepted by the gas, *we can augment the absolute value of that fraction by augmenting the total of which it is a fraction.*

METHOD OF COMPENSATION. COMPLETE APPARATUS.

The problem, happily, admits of an effective practical solution. You know that when we allow heat to fall upon the opposite faces of the thermo-pile, the currents generated neutralise each other more or less; and, if the quantities of heat falling upon the two faces be perfectly equal, the neutralisation is complete. Our galvanometer

needle is now deflected to 80° by the flux of heat passing through the tin cylinder tube (fig. 100); I uncover the second face of the pile, which is also furnished with a conical reflector, and place a second cube of boiling water in front of it; the needle, as you see, descends instantly.

By means of a proper adjusting screen the quantity of heat falling upon the posterior face of the pile can be so regulated that it shall exactly neutralise the heat incident upon its other face: this is now effected; and the needle points to zero.

Here, then, we have two powerful and perfectly equal fluxes of heat, falling upon the opposite faces of the pile, one of which passes through our exhausted cylinder. If air be allowed to enter the cylinder, and if this air exert any appreciable action upon the rays of heat, the equality now existing will be destroyed; a portion of the heat passing through the tube being intercepted by the air, the second source of heat will triumph; the needle, now in its most sensitive position, will be deflected; and from the magnitude of the deflection we can accurately calculate the absorption.

I have thus sketched, in rough outline, the apparatus by which our researches on the relation of radiant heat to gaseous matter must be conducted. The necessary tests are, however, at the same time so powerful and so delicate, that a rough apparatus like that just described would not answer our purpose. But you will now experience no difficulty in comprehending the construction and application of the more perfect apparatus, with which the experiments on gaseous absorption and radiation have been actually made.

Between S and S′ (Plate I. frontispiece) stretches the *experimental cylinder*, a hollow tube of brass, polished within; at S and S′ are the plates of rock-salt which close the cylinder air-tight; the length from S to S′, in the ex-

periments to be first recorded, is 4 feet. The source of heat, C, is a cube of cast copper, filled with water, which is kept continually boiling by the lamp L. Attached to the cube C by brazing is the short cylinder F, of the same diameter as the experimental cylinder, and capable of being connected air-tight with the latter at S. Thus, between the source C and the end S of the experimental tube, we have *the front chamber* F, from which the air can be removed so that the rays from the source will enter the cylinder S S′ unsifted by air. To prevent the heat of the source C from passing by conduction to the plate at S, the chamber F is caused to pass through the vessel V, in which a stream of cold water continually circulates, entering through the pipe *i i*, which dips to the bottom of the vessel, and escaping through the waste-pipe *e e*. The experimental tube and the front chamber are connected, independently, with the air-pump A A, so that either of them may be exhausted or filled, without interfering with the other. I may remark that, in later arrangements, the experimental cylinder was supported apart from the pump, being connected with the latter by a flexible tube. The tremulous motion of the pump, which occurred when the connection was rigid, was thus completely avoided. P is the thermo-pile, placed on its stand at the end of the experimental cylinder, and furnished with its two conical reflectors. C′ is the *compensating cube*, used to neutralise the radiation from C; H is the *adjusting screen*, which is capable of an exceedingly fine motion to and fro. N N is a delicate galvanometer connected with the pile P, by the wires *w w*. The graduated tube O O (to the right of the plate), and the appendage M K (attached to the centre of the experimental tube) shall be referred to more particularly by-and-by.

It would hardly sustain your interest, were I to state the difficulties which at first beset the investigation con-

ducted with this apparatus, or the numberless precautions, which the exact balancing of the two powerful sources of heat here resorted to, render necessary. I believe the experiments, made with atmospheric air alone, might be numbered by tens of thousands. Sometimes for a week, or even for a fortnight, coincident and satisfactory results would be obtained; the strict conditions of accurate experimenting would appear to be found, when an additional day's experience would destroy the superstructure of hope and necessitate a recommencement, under changed conditions, of the whole inquiry. It is this which daunts the experimenter: it is this preliminary fight with the entanglements of a subject, so dark, so doubtful, so uncheering, —without any knowledge whether the conflict is to lead to anything worth possessing,—which renders discovery difficult and rare. But the experimenter, especially the *young* experimenter, ought to know that, as regards his own moral manhood, he cannot but win, if he only contend aright. Even with a negative result, the consciousness that he has gone fairly to the bottom of his subject, as far as his means allowed—the feeling that he has not shunned labour, though that labour may have resulted in laying bare the nakedness of his case—reacts upon his own mind, and gives it firmness for future work.

But to return;—I first neglected atmospheric vapour and carbonic acid altogether; concluding, as others afterwards did, that the quantities of these substances being so small, their effect upon radiant heat must be quite inappreciable; after a time, however, this assumption was found to be leading me quite astray. Chloride of calcium was first used as a drying agent, but I had to abandon it. Pumice stone, moistened with sulphuric acid, was next used, but it also proved unsuitable. I finally resorted to pure glass broken into small fragments, wetted with pure sulphuric acid, and inserted by means of a funnel into a

U-tube. This arrangement was found to be the best, but even here the greatest care was needed. It was necessary to cover each column of the U-tube with a layer of dry glass fragments; for the smallest particle of dust from the cork, or a quantity of sealing-wax not more than the twentieth part of a pin's head in size, was quite sufficient, if it reached the acid, to vitiate the results. The drying-tubes, moreover, had to be frequently changed, as the organic matter of the atmosphere, infinitesimal though it was, after a time introduced disturbance.

To remove the carbonic acid, pure Carrara marble was broken into fragments, wetted with caustic potash, and introduced into a U-tube. These, then, are the agents which I now employ for drying the gas and removing the carbonic acid; but previous to their final adoption, an arrangement shown in Plate I. was made use of. The glass tubes marked Y Y, each three feet long, were filled with chloride of calcium, after them were placed two U-tubes, R, Z, filled with pumice stone and sulphuric acid. Hence, the air, in the first place, had to pass over 18 feet of chloride of calcium, and afterwards through the sulphuric acid tubes, before entering the experimental tube S S′. A gasholder, G G′, was employed for other gases than atmospheric air. In later investigations, this arrangement, as already stated, is abandoned, a simpler one being found more effectual.

Both the front chamber F, and the experimental tube S S′ being exhausted, the rays pass from the source C through the front chamber; across the plate of rock-salt at S, through the experimental tube, across the plate at S′, afterwards impinging upon the anterior surface of the pile P. This radiation is neutralised by that from the compensating cube C′. The needle, you will observe, is at zero. We will commence our experiments by applying this severe test to dry air. It is now entering the ex-

perimental cylinder; but you see no motion of the needle, and thus our more powerful mode of experiment fails to detect any absorption on the part of the air. Its atoms, apparently, are quite incompetent to stop the calorific waves; *air is a practical vacuum, as regards the rays of heat.* Oxygen, hydrogen, and nitrogen, when carefully purified, exhibit the action of atmospheric air; they are all sensibly neutral.

RADIATION THROUGH OLEFIANT GAS.

This is the deportment which, prior to the researches now to be described, was ascribed to gases generally. It was to open this vast field that my efforts were directed at the beginning of 1859. Your patience would hardly bear the strain were I to conduct you along the devious line of experiment and verification pursued throughout the inquiry; and I will therefore at once choose a substance which will demonstrate how powerful the action of purely gaseous matter on radiant heat may be. This gas-holder contains olefiant gas, the perfect transparency of which to light is demonstrated by discharging it into the air; you see nothing, the gas is not to be distinguished from the air. The experimental tube is now exhausted, and the needle points to zero. Permitting the olefiant gas to enter, the needle moves in a moment; the invisible gas intercepts the heat, like a solid opaque body—the final and permanent deflection, when the tube is full of gas, amounting to 70°.

I now interpose a metal screen between the pile P and the end S′ of the experimental tube, thus entirely cutting off the radiation through the tube. The face of the pile turned towards the metal screen wastes its heat speedily by radiation; it falls rapidly to the temperature of this room, the radiation from the compensating cube alone acting on

the pile, and producing a deflection of 75°. At the commencement of the experiment the radiations from both cubes being equal, the deflection 75° corresponds to the *total radiation* through the exhausted experimental tube.

Taking as unit the quantity of heat necessary to move the needle from 0° to 1°, the number of units expressed by a deflection of 75° is

276.

The number of units expressed by a deflection of 70° is

211.

Out of a total, therefore, of 276, olefiant gas has intercepted 211; that is, about seven-ninths of the whole, or about 80 per cent.

It might occur to you, as it at first occurred to me, that an opaque layer of some kind had been suddenly precipitated on our plates of salt, when the gas entered. The substance, however, deposits no such layer. When a current of the dried gas is discharged against a polished plate of salt, you do not perceive the slightest dimness. The rock-salt plates, moreover, though necessary for exact measurements, are not necessary to show the destructive power of this gas. Here is an open tin cylinder, interposed between the pile and our radiating source; when olefiant gas is forced gently into the cylinder from this gasholder, you see the needle fly up to its stops. Observe the smallness of the quantity of gas now employed. First cleansing the open tube, by forcing a current of air through it, and bringing the needle to zero; I turn a cock on and off, as speedily as possible. A mere bubble of the gas enters the tube in this brief interval; still you see that its presence causes the needle to swing to 70°. Let us now abolish the open tube, and leave nothing but the free air between the pile and the source. From the gasometer I discharge olefiant gas into this open space. You see

nothing in the air; but the swing of the needle through an arc of 60° declares the presence of this invisible barrier to the calorific rays.

Thus, it is shown that the ethereal undulations which pass among the atoms of oxygen, nitrogen, and hydrogen, without hindrance, are powerfully intercepted by the molecules of olefiant gas. We shall find other transparent gases, also, almost immeasurably superior to air. We can limit at pleasure the number of the gaseous molecules, and thus vary the amount of destruction of the ethereal waves. Attached to the air-pump is a barometric tube, by means of which measured portions of the gas can be admitted. The experimental cylinder is now exhausted: turning a cock slowly on, and observing the mercury gauge, the olefiant gas enters, till the mercurial column has been depressed an inch. I observe the galvanometer, and read the deflection. Another inch being added, the absorption effected by gas under two inches of pressure is determined. Proceeding thus, we obtain for pressures from 1 to 10 inches the following absorptions:—

Olefiant Gas.

Pressure in inches	Absorption
1	90
2	123
3	142
4	157
5	168
6	177
7	182
8	186
9	190
10	193

The unit here used is the amount of heat absorbed, by a *whole atmosphere* of dried air. The table, for example, shows that one-thirtieth of an atmosphere of olefiant gas exercises ninety times the absorption of

a whole atmosphere of air. The deflection produced by the tubeful of dry air is here taken to be one degree: it is probably even less than this infinitesimal amount.

The table also informs us that each additional inch of olefiant gas produces less effect than the preceding one. A single inch, at the commencement, intercepts 90 rays, but a second inch absorbs only 33 additional rays, while the addition of an inch, when nine inches are already in the tube, effects the destruction of only 3 rays. This is what might reasonably be expected. The number of rays emitted is finite, and the discharge of the first inch of olefiant gas amongst them has so thinned their ranks, that the execution produced by the second inch is naturally less than that of the first. This execution must diminish, as the number of rays capable of being destroyed by the gas becomes less; until, finally, all absorbable rays being removed, the residual heat passes through the gas unimpeded.[1]

But supposing the quantity of gas first introduced to be so inconsiderable, that the heat intercepted by it is a vanishing quantity, compared with the total amount, we might then reasonably expect that, for some time at least, the quantity of heat intercepted would be proportional to the quantity of gas present—that a double quantity of gas would produce a double effect, a treble quantity a treble effect; or, in general terms, that the absorption would, for a time, be found proportional to the density.

[1] A wave of ether starting from a radiant point in all directions, in a uniform medium, constitutes a spherical shell, which expands with the velocity of light or of radiant heat. A *ray* of light, or a *ray* of heat, is a line perpendicular to the wave, and, in the case here supposed, the rays would be the radii of the spherical shell. The word 'ray,' however, is used in the text, to avoid circumlocution, as equivalent to the term *unit of heat*. Thus, calling the amount of heat intercepted by a whole atmosphere of air 1, the amount intercepted by $\frac{1}{30}$th of an atmosphere of olefiant gas is 90.

To test this idea, we will make use of a portion of the apparatus omitted in the general description. O O (Plate I.) is a graduated glass tube, the end of which dips into the basin of water B. The tube is closed above by means of the stopcock r; $d\ d$ is a tube containing fragments of chloride of calcium which dries the gas. The tube O O is first filled with water up to the cock r, and the water is afterwards carefully displaced by olefiant gas, introduced in bubbles from below. The gas is then admitted into the experimental cylinder, and as it enters, the water rises in O O, each of the divisions of which represents a volume of $\frac{1}{50}$th of a cubic inch. Successive measures of this capacity are permitted to enter the tube, and the absorption in each particular case is determined.

In the following table, the first column contains the quantity of gas admitted into the tube; the second contains the corresponding absorption; the third column contains the absorption, calculated on the supposition that it is proportional to the density.

Olefiant Gas.

Unit-measure, $\frac{1}{50}$th of a cubic inch.

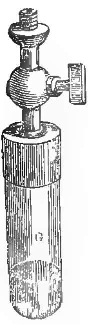
Measures of Gas	Absorption: Observed	Absorption: Calculated
1	2·2	2·2
2	4·5	4·4
3	6·6	6·6
4	8·8	8·8
5	11·0	11·0
6	12·0	13·2
7	14·8	15·4
8	16 8	17·6
9	19·8	19·8
10	22·0	22·0
11	24·0	24·2
12	25·4	26·4
13	29·0	28·6
14	30·2	29 8
15	33·5	33·0

This table proves the correctness of the surmise, that when very small quantities of the gas are employed, the absorption is sensibly proportional to the density. But consider for a moment the tenuity of the gas with which we have here operated. The volume of our experimental tube is 220 cubic inches; imagine $\frac{1}{50}$th of a cubic inch of gas diffused in this space, and you have the atmosphere through which the calorific rays passed in our first experiment. This atmosphere possesses a pressure not exceeding $\frac{1}{11000}$th that of ordinary air. It would depress the mercurial column connected with the air-pump not more than $\frac{1}{367}$th of an English inch. Its action, however, upon the calorific rays is perfectly distinct, being more than twice that of a whole atmosphere of dry air.

RADIATION THROUGH ETHER VAPOUR.

But the absorptive energy of olefiant gas, extraordinary as it is shown to be by the foregoing experiments, is exceeded by that of various vapours, the action of which on radiant heat is now to be illustrated. This glass flask, G (fig. 101), is provided with a brass cap, into which a stopcock can be screwed air-tight. A small quantity of sulphuric ether is poured into the flask, and by means of an air-pump, the air which fills the flask above the liquid is completely removed. I attach the flask to the experimental tube, which is now exhausted —the needle pointing to zero—and permit the vapour from the flask to enter it. The mercury of the gauge sinks, and when it is depressed one inch, the further supply of vapour is stopped. The moment the vapour entered, the needle moved, and it now points to 65°. I can add another inch, and again determine the absorption; a third inch, and do the same. The absorp-

FIG. 101.



tions effected by four inches, introduced in this way, are given in the following table. For the sake of comparison, the corresponding absorptions of olefiant gas are placed in the third column.

Sulphuric Ether.

Pressure in inches of mercury	Absorption	Corresponding absorption of olefiant gas
1	214	90
2	282	123
3	315	142
4	330	154

For these pressures the absorption of radiant heat by the vapour of sulphuric ether is about two and two-third times the absorption by olefiant gas. There is, moreover, no proportionality between the quantity of vapour and the absorption.

But reflections similar to those which we have already applied to olefiant gas are also applicable to sulphuric ether. Supposing we make our unit-measure small enough, the number of rays first destroyed will vanish in comparison with the total number, and probably, for a time, the absorption will be directly proportional to the density. To examine whether this is the case, the other portion of the apparatus, omitted in the general description, was made use of. K (Plate I.) is one of the small flasks described a moment ago (fig. 101), with a brass cap, which is closely screwed on to the stopcock c'. Between the cocks c' and c, the latter of which is connected with the experimental tube, is the chamber M, the capacity of which is accurately determined. The flask K is partially filled with ether, the air above the liquid and that dissolved in it being removed. The tube S S′ and the chamber M being exhausted, the cock c is shut off; and c' being turned on, the chamber M is filled with pure ether vapour. By turning c' off and c on, this quantity of

vapour is allowed to diffuse itself through the experimental tube, where its absorption is determined; successive measures are thus sent into the tube, and the effect produced by each is noted.

In the following table, the unit-measure made use of had a volume of $\frac{1}{100}$th of a cubic inch.

Sulphuric Ether.

Unit-measure, $\frac{1}{100}$th of a cubic inch.

Measures	Absorption Observed	Absorption Calculated
1	5·0	4·6
2	10·3	9·2
4	19·2	18·4
5	24·5	23·0
6	29·5	27·0
7	34·5	32·2
8	38·0	36·8
9	44·0	41·4
10	46·2	46·2
11	50·0	50·6
12	52·8	55·2
13	55·0	59·8
14	57·2	64·4
15	59·4	69·0

We here find that the proportion between density and absorption holds sensibly good for the first eleven measures, after which the deviation from proportionality gradually aumgents.

No doubt, for smaller measures than $\frac{1}{100}$th of a cubic inch, the above law holds still more rigidly true; and in a suitable locality it would be easy to determine, with perfect accuracy, $\frac{1}{10}$th of the absorption produced by the first measure; this would correspond to $\frac{1}{1000}$th of a cubic inch of vapour. But, before entering the tube, the vapour had only the tension due to the temperature of the laboratory, namely 12 inches. This would require to be

multiplied by 2·5 to bring it up to that of the atmosphere. Hence, the $\frac{1}{1000}$th of a cubic inch would, on being diffused through a tube possessing a capacity of 220 cubic inches, have a pressure of $\frac{1}{220} \times \frac{1}{2.5} \times \frac{1}{1000} = \frac{1}{550000}$th of an atmosphere. That the action of a transparent vapour so attenuated upon radiant heat should be at all measurable is simply astounding.

These experiments with ether and olefiant gas show that not only do gaseous bodies, at the ordinary atmospheric pressure, offer an impediment to the transmission of radiant heat—not only are the interstitial spaces of such gases incompetent to allow the ethereal undulations free passage—but, also, that after their density has been rendered infinitesimal, the door thus opened is not wide enough to let all the undulations through. There is something in the constitution of the individual molecules, thus sparsely scattered, which enables them to take up the motion of the ethereal waves. Through dry air the heat-rays pass without sensibly warming it; through olefiant gas and ether vapour they cannot pass thus freely; but every wave withdrawn from the radiant beam produces its equivalent motion in the body of the absorbing gas, and raises its temperature. It is, therefore, a case of transference, not of annihilation.

Before changing the source of heat here employed, let us direct our attention for a moment to the action of a few more of the permanent gases on radiant heat. To measure the quantities introduced into the experimental tube, the mercury gauge of the air-pump is employed. In the case of carbonic oxide, the following absorptions correspond to the pressures annexed to them; the action of a full atmosphere of air, assumed to produce a deflection of one degree, being taken as unity:—

Carbonic Oxide.

Pressure in inches of mercury	Absorption: Observed	Absorption: Calculated
0·5	2·5	2·5
1·0	5·6	5·0
1·5	8·0	7·5
2·0	10·0	10·0
2·5	12·0	12·5
3·0	15·0	15·0
3·5	17·5	17·5

As in the former cases, the third column is calculated on the assumption that the absorption is directly proportional to the density of the gas; and we see that for seven measures, or up to a pressure of 3·5 inches, the proportionality holds strictly good. But for large quantities this is not the case. When, for instance, the unit-measure is 5 inches, instead of half an inch, we obtain the following result:—

Pressures in inches	Absorption: Observed	Absorption: Calculated
5	18	18
10	32·5	36
15	45	54

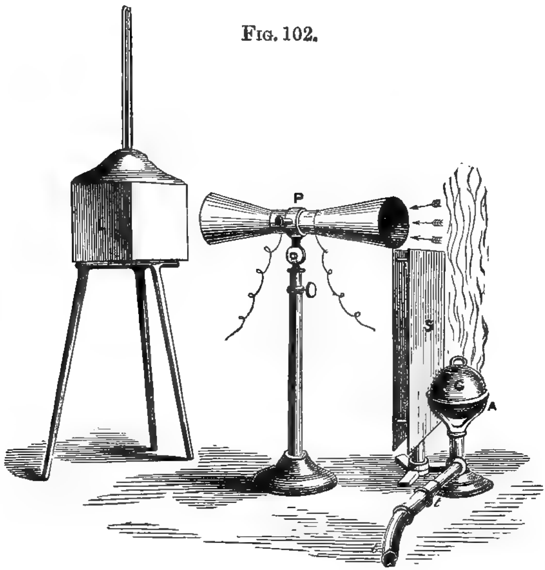
Carbonic acid, sulphide of hydrogen, nitrous oxide, and other gases, though differing in the energy of their absorption, and all of them exceeding carbonic oxide, exhibit when small and large quantities are used, a similar deportment towards radiant heat.

Thus, then, while in the case of some gases, we find an almost absolute incompetence on the part of their atoms to intercept the ethereal waves, the molecules of other gases, struck by these same undulations, absorb their motion, and become themselves centres of heat. We have now to examine whether these atoms and molecules which can accept motion from the ether in such very different degrees, are not also characterised by their competency to

impart motion to the ether in different degrees; or, to use the common language, having learnt something of the power of different gases, as *absorbers* of radiant heat, we have now to inquire into their capacities as *radiators*.

An arrangement is before you by means of which we can test the general principle. Finer and more far-reaching combinations will be made later on. P (fig. 102) is the thermo-pile, with its two conical reflectors; S is a

FIG. 102.



double screen of polished tin; A is an argand burner, consisting of two concentric perforated rings; C is a copper ball which, during the experiments, is heated under redness; while the tube $t\ t$ leads to a gasholder. When the hot ball C is placed on the burner, it warms the air in contact with it; an ascending current is thus established, which, to some extent, acts upon the pile. To neutralise this action, a large Leslie's cube, L, filled with water, a few

degrees above the air in temperature, is placed before the opposite face of the pile. The needle being thus brought to zero, the gas to be experimented on is forced, by a gentle water pressure, through the orifices of the burner; it meets the ball *c*, glides along its surface, and ascends in a warm current, in front of the pile. The rays from the heated gas issue in the direction of the arrows, against the pile, and the consequent deflection of the galvanometer needle indicates the magnitude of the radiation.

The results of the experiments are given in the column of the following table headed 'Radiation;' the numbers there recorded marking the extreme limits to which the needle swung, when the rays from the gas fell upon the pile:—

	Radiation.	Absorption.
Air	Insensible	Insensible
Oxygen	"	"
Nitrogen	"	"
Hydrogen	"	"
Carbonic oxide	12°	18°
Carbonic acid	18	25
Nitrous oxide	29	44
Olefiant gas	53	61

In the second column of figures, under the head 'Absorption,' are placed the deflections due to the absorption of the gases here employed, at a common tension of 5 inches. A comparison of the two columns shows us that radiation and absorption go hand in hand; that the molecule which is competent to *intercept* radiant heat, is competent in a proportionate degree to *generate* radiant heat. That, in short, a capacity to accept motion from the luminiferous ether, and to impart motion to it, by gaseous bodies, are correlative properties.

And here, be it remarked, we are relieved from all considerations regarding the influence of cohesion on the results. In solids and liquids the particles are more or less in thrall, and cannot be considered as individually

free. The difference in point of radiative and absorptive power, between alum and rock-salt, for example, might be fairly regarded as due to their character as aggregates, held together by crystallising force. But the difference between olefiant gas and atmospheric air cannot be explained in this way; it is a difference dependent on the individual molecules of these substances; and thus, our experiments with gases and vapours probe the question of atomic constitution to a depth quite unattainable with solids and liquids.

FURTHER EXPERIMENTS ON GASES.

I have refrained, thus far, from giving you as full a tabular statement of the absorptive powers of gases and vapours as the experiments made with the apparatus thus far described would enable me to do; knowing that results obtained with another apparatus were in reserve, which would better illustrate the subject. This second arrangement is the same in principle as the first, only two changes of importance being made in it. The first is that, instead of making a cube of boiling water the source of heat, a plate of copper is employed, against which a thin steady gas-flame from a Bunsen's burner is caused to play, raising the temperature of the plate to about 270° C. The heated plate forms the back of our new front chamber, which latter can be exhausted independently, as before. The second alteration is the substitution of a tube of glass of the same diameter, and 2 feet 8 inches long, for the tube of brass S S′, Plate I. All the other parts of the apparatus remain as before. The gases were introduced in the manner already described into the experimental tube, and from the galvanometric deflection consequent on the entrance of each gas, its absorption was calculated.

The following table gives the relative absorptions of several gases, at a common pressure of one atmosphere. It may be remarked that the differences between air and the other gases would be still greater if the brass tube had been employed; but the use of it would have excluded the corrosive gases mentioned in the table.

Name	Absorption at 30 inches pressure
Air	1
Oxygen	1
Nitrogen	1
Hydrogen	1
Chlorine	39
Hydrochloric acid	62
Carbonic oxide	90
Carbonic acid	90
Nitrous oxide	355
Sulphide of hydrogen	390
Marsh gas	403
Sulphurous acid	710
Olefiant gas	970
Ammonia	1195

The most powerful and delicate tests yet applied have not enabled me to establish a difference between oxygen, nitrogen, hydrogen, and air. The absorption of these substances is exceedingly small—probably even smaller than I have assumed it. The more perfectly the above-named gases are purified, the more closely does their action approach to that of a vacuum. And who can say that the best drying apparatus is perfect? We cannot even say that sulphuric acid, however pure, may not yield a modicum of vapour to the gases passing through it, and thus make the absorption by those gases appear greater than it ought. Stopcocks also must be greased, and hence may contribute an infinitesimal impurity to the air passing through them. But however this may be, it is certain that if any further advance should be made in the purification of the more feebly acting gases, it will only

serve to augment the enormous differences of absorption here exhibited.

Ammonia, at the tension of an atmosphere, exerts an absorption at least 1,195 times that of air. When a metal screen is interposed between the pile and the experimental tube filled with this gas, the needle moves a little, but so little that, unless quite close, you entirely fail to see it. What does this prove? It proves that the ammonia which, within our glass tube, is as transparent to light as the air we breathe, is so opaque to the heat radiating from our source, that the addition of a plate of metal hardly augments its opacity. There is, indeed, reason to believe that a layer three feet in depth of this light transparent gas, is really as black to the invisible calorific rays, as if the experimental tube were filled with ink, pitch, or any other impervious substance.

With oxygen, nitrogen, hydrogen, and air, the action of a whole atmosphere is so small, that it would be quite useless to attempt to determine the action of a fractional part of an atmosphere. Could we, however, make such a determination, the difference between them and the other gases would come out still more forcibly than in the last table. In the case of the energetic gases, we know that the calorific rays are most copiously absorbed by the portion of gas which first enters the experimental tube; the quantities which enter last, producing, in many cases, a merely infinitesimal effect. If, therefore, instead of comparing the gases at a common pressure of one atmosphere, we were to compare them at a common pressure of an inch, we should doubtless find the difference between the least absorbent and the most absorbent gases greatly augmented. We have already learned that, when the absorption is small, the quantity absorbed is proportional to the amount of gas present. Assuming this to be true for air, and for the other feeble gases referred to; taking, that is, their

absorption at 1 inch of pressure to be $\frac{1}{30}$th of that at 30 inches, and calling this unity; we have the following comparative results. It will be understood that in every case, except the first four, the absorption of 1 inch of the gas was determined by direct experiment:—

Name	Relative absorption at 1 inch pressure
Air	1
Oxygen	1
Nitrogen	1
Hydrogen	1
Chlorine	60
Bromine	160
Carbonic oxide	750
Carbonic acid	972
Hydrobromic acid	1005
Nitric oxide	1590
Nitrous oxide	1860
Sulphide of hydrogen	2100
Ammonia	5460
Olefiant gas	6030
Sulphurous acid	6480

What extraordinary differences in the constitution and character of the molecules of various gases do the above results reveal! For every ray intercepted by air, oxygen, hydrogen, or nitrogen—ammonia intercepts 5,460; olefiant gas 6,030; while sulphurous acid destroys 6,480. With such results before us, we can hardly help trying, with the eye of intellect, to discern the physical qualities on which these vast differences depend. Is the hope unwarranted, that we may ultimately make radiant heat such a *feeler* of atomic constitution, that we shall be able to infer from its action, the mechanism of the molecules themselves?

Have we, even now, no glimpse of a relation between absorption and atomic constitution? You remember our experiments with gold, silver, and copper; how feebly they radiated and how feebly they absorbed. We heated them by

boiling water; that is to say, we imparted, by the contact of the hot water, motion to their atoms; and found this motion to be transferred with extreme slowness to the luminiferous ether. That the atoms of these bodies move with scarcely any resistance through the ether which surrounds them, may also be inferred from the length of time which they require to cool in vacuo. But we have seen that when the motion which the atoms possess, and which they are incompetent to transfer to the ether, is imparted, by contact, to a coat of varnish, or of chalk, or of lampblack, or even to flannel or velvet, glass or earthenware, these bodies, being good radiators, rapidly transfer the motion.

In what respect do those good radiators differ from the metals referred to? In one profound particular—the metals *are elements*; the others *are compounds*. In the metals, the atoms vibrate singly; in the varnish, velvet, earthenware, and glass, they vibrate in groups. And now, in bodies as diverse from the metals as can possibly be conceived, we find the same significant fact making its appearance. Oxygen, hydrogen, nitrogen, and air, are elements, or mixtures of elements, and, both as regards radiation and absorption, their feebleness is declared. They vibrate in the ether, with scarcely any loss of energy.

It is impossible not to be struck by the position of chlorine and bromine in the last table. Chlorine is an extremely dense and also a coloured gas; bromine is a far more densely-coloured vapour; still we find them, as regards perviousness to the heat of our source, standing above every transparent compound gas in the table. The act of combination with hydrogen produces, in the case of each of these substances, a transparent compound; but the chemical act, which augments the transparency to light, augments the opacity to invisible heat. Hydro-

chloric acid absorbs more than chlorine; and hydrobromic acid absorbs more than bromine.

Further, the element bromine is here in the liquid condition. Placed in this glass cell it cuts off utterly the flame of a lamp or candle. But when a candle is placed in front of the cell, and a thermo-electric pile behind it, the prompt movement of the needle declares the passage of radiant heat through the bromine. This heat consists entirely of the obscure rays of the candle, for the light, as stated, is utterly cut off. Let us remove the candle, and put in its place a copper ball, heated not quite to redness. The needle at once flies to its stops, showing the transparency of the bromine to the heat emitted by the ball. It is impossible, I think, to close our eyes against this convergent evidence that the free atoms swing with ease in the ether, while when grouped by chemical union into oscillating systems, they cause its waves to swell; imparting to it an amount of motion quite beyond their power to communicate as long as they remained uncombined.[1]

But it will occur to you, no doubt, that lampblack, which is an elementary substance, is one of the best absorbers and radiators in nature. Let us examine this substance a little. Ordinary lampblack contains many impurities; it has various hydrocarbons condensed within it, and these hydrocarbons are powerful absorbers and radiators. Lampblack, therefore, as hitherto applied, can hardly be considered an element at all. I have, however, had the hydrocarbons in great part removed, by carrying through red-hot lampblack a current of chlorine gas: but the purified substance still continued to be a powerful radiator and absorber. Well what *is* lampblack?

[1] I should like to reserve my opinion as to the comparative strength of the radiation of the molecule as a whole, and that of its constituent atoms.

Chemists will tell you that it is an allotropic form of the diamond: here, in fact, is a diamond reduced to charcoal by intense heat. Now the allotropic condition has long been defined as due to a difference of molecular arrangement; hence, it is conceivable that this arrangement, which causes such a marked physical difference between lampblack and diamond, may consist of an atomic grouping, which causes the body to act on radiant heat as if it were a compound. Such an arrangement of an element, though exceptional, is quite conceivable; and it will afterwards be shown that this is actually and eminently the case as regards ozone—an allotropic form of our highly ineffectual oxygen.

But, in reality, lampblack is not so impervious as you might suppose it to be. Melloni has shown it to be transparent, in an unexpected degree, to radiant heat emanating from a low source, and the experiment now to be performed will more than corroborate his. This plate of rock-salt, by being held over a smoky lamp, has been so thickly coated with soot that it does not allow a trace of light from the most brilliant gas jet to pass through it. Between the smoked plate and a vessel of boiling water, which is to serve as our source of heat, I place a screen, the thermo-electric pile being at the other side of the smoked plate. When the screen is withdrawn, the needle moves from zero, its final and permanent deflection being 52°. I now cleanse the salt perfectly, and determine the radiation through the unsmoked plate—the deflection is 71°. But the value of the deflection 52°, expressed with reference to our usual unit, is 85, and the value of 71°, or the total radiation, is about 222. Hence, the whole radiation is to the radiation through the soot as

$$222 : 85 = 100 : 38$$

that is to say, 38 per cent. of the incident heat has been

transmitted by the layer of lampblack. We shall have to deal subsequently with far more impressive illustrations of the diathermancy of opaque bodies than that here exhibited by lampblack.

FURTHER EXPERIMENTS ON VAPOURS.

I have now to bring before you some new experiments on vapours, executed with the apparatus last described. A number of flasks were prepared, of the shape and size of common test-tubes, each of which was provided with a screw-cap carefully cemented on to it, and by means of which it could be attached to a stopcock, and thus connected with the experimental tube. The mode of operation was this: The liquid was introduced into the flask by means of a small glass funnel. A stopcock was then attached to the flask, and connected with a second air-pump, kept always at hand. The air above the liquid was removed, and the air dissolved in it allowed to bubble away, until nothing remained but the pure liquid below and the pure vapour above it. The stopcock was then shut off, and the flask united to the experimental tube. The exhaustion of the experimental tube being complete, and the needle of the galvanometer at zero, the cock attached to the flask was turned on, and the mercury gauge carefully observed at the same time. No bubbling of the liquid was in any case permitted. The vapour entered silently and without the slightest commotion; and when the mercurial column was depressed to the extent required, the vapour was promptly intercepted.

The energy with which the needle moves the moment a strong vapour enters is so extraordinary that, lest the shock against them should derange the magnetism of the astatic pair, I have had to remove the stops which arrested the swing of the needle at 90°. It often swings far beyond

a quadrant; and after it has come finally and permanently to rest, its position is observed in the following manner: The dial of the galvanometer being well illuminated, a looking-glass is placed behind the instrument, at such an angle that when looked at horizontally the image of the dial is clearly seen. This image is observed by a good telescope fixed at a distance of 11 feet from the galvanometer. Attached to the needle and in continuation of it, is a bit of glass fibre of extreme fineness, blackened with Indian ink. This index ranges over the graduated circle, and by means of it a very small fraction of a degree can be easily read off. Previous experiments had taught me that it was dangerous to go too near the delicately suspended needle. In this way, the vapours of the substances mentioned in the next table have been examined at pressures of 0·1, 0·5, and 1 inch, respectively.

	Absorption of Vapours at the pressures		
	0·1	0·5	1·0
Bisulphide of carbon	15	47	62
Iodide of methyl	35	147	242
Benzol	66	182	267
Chloroform	85	182	236
Methylic alcohol	109	390	590
Amylene	182	535	823
Sulphuric ether	300	710	870
Alcohol	325	622	
Formic ether	480	870	1075
Acetic ether	590	980	1195
Propionate of ethyl	596	970	
Boracic ether	620		

Let us compare some of the results recorded in this table of transparent vapours with the action of the dark coloured vapour of bromine. The absorption of bromine vapour at 1 inch pressure is about 6, and at 0·1 of an inch pressure would probably not exceed 1; hence at 0·1 of an inch pressure, bisulphide of carbon exerts probably 15 times the absorbent power of bromine; but bisulphide of

carbon is the feeblest of the compound vapours hitherto discovered. The strongest of these, boracic ether, has, according to the above estimate, and at the pressure stated, more than 600 times the absorbing energy of the strongly-coloured bromine.

The whole of the numbers in the above table are referred to atmospheric air as unity; 0·1 of an inch of bisulphide of carbon vapour, for example, absorbs 15 times as much as a whole atmosphere of air. Let us compare, for an instant, the action of boracic ether with that of air. We arrive at an approximate comparison in this way. The absorption of the tenth of an inch of boracic ether is something more than that of a whole inch of methylic alcohol; by diminishing the quantity of methylic alcohol to one-tenth, we reduce its absorption from 590 to 109. The absorption of one-tenth of an inch of boracic ether is 620°; suppose its absorption to diminish with diminished quantity in the proportion of methylic alcohol, we should then have for 0·01 of an inch of boracic ether an absorption of 111; that is to say, for $\frac{1}{3000}$th of an atmosphere of boracic ether, we should have an action 111 times that of a whole atmosphere of oxygen, nitrogen, hydrogen, or atmospheric air.

With the transparent elementary gases it is impossible to measure directly the absorption of 0·1 of an inch; but assuming, as before, that up to an absorption of 1 the effect is proportional to the quantity of gas present, the absorption of each of the elementary gases, at a pressure of 0·1 of an inch, would be about 0·0033; hence the absorption of boracic ether at 0·1 of an inch pressure is to that of air at the same pressure as

$$620 : 0{\cdot}0033,$$

which would give to the ether an absorbing energy 186,000 times that of air

I have already spoken of the blackness of ammonia at 30 inches pressure. Referring to the table on page 346, its absorption is found to be 1195. In the last table the vapour of acetic ether, under only one-thirtieth of the pressure of the ammonia, produces apparently the same effect; its absorption is also 1195. Such facts give one entirely new ideas of the capabilities of gaseous matter; and our wonder will not be diminished by the results to be recorded further on.

At an early stage of these inquiries, the danger of a change of reflective power occurring on the interior surface of the experimental tube was forced on my attention. Wishing to try the action of chlorine on radiant heat, I admitted a quantity of the gas into the polished tube. The needle was deflected with prompt energy, but on pumping out it refused to return to its first position. To cleanse the tube dry air was introduced into it ten times in succession; but the needle continued to point persistently to the 40th degree from zero. The cause was at once surmised; the chlorine had attacked the polished metal and partially destroyed its reflecting power, the stoppage of the radiant heat by the sides of the tube itself being the cause of the observed deflection. For subsequent experiments the interior of the tube had to be carefully repolished.

Some of those who have followed me in these researches do not appear to be quite aware of the labour of verification expended on them; otherwise, I imagine, greater caution would have been employed in the repetition of the experiments. In reference to the present point, it immediately occurred to me that, though no gas or vapour previously examined had produced a permanent effect of the kind observed with chlorine, it was necessary to be perfectly sure that a change of reflective

power within the tube had not vitiated the other results. With the permanent gases, this suspicion was immediately set at rest: but a real cause of anxiety was the possible precipitation of the vapours examined on the polished interior surface of the tube. Such precipitation might damage the reflector without producing the permanent effect of chlorine. I therefore resolved to abolish the reflecting surface, and test the results with unreflected heat. A length of two feet of the brass experimental tube was therefore coated carefully on the inside with lampblack, so as practically to destroy its reflective power. With it were determined the absorptions of all the vapours previously examined, at a common pressure of 0·3 of an inch. A general corroboration was all here aimed at, and I am satisfied that the slight discrepancies which the measurements exhibit would disappear, or be accounted for, by a still more careful examination.

In the following table the results obtained with the blackened tube and with the polished tube are placed side by side, the pressure in the former case being three-tenths and in the latter five-tenths of an inch.

Vapour	Absorption per 100: Blackened Tube 0·3 pressure	Absorption per 100: Bright Tube 0·5 pressure	
Bisulphide of carbon	5	21	23
Iodide of methyl	15·8	60	71
Benzol	17·5	78	79
Chloroform	17·5	89	79
Iodide of ethyl	21·5	94	97
Wood-spirit	26·5	123	120
Methylic alcohol	29	133	131
Chloride of amyl	30	137	135
Amylene	31·8	157	143

The order of absorption is here shown to be exactly the same in both tubes, the quantity intercepted in the bright tube being in general about $4\frac{1}{2}$ times that absorbed in the black one. In the third column, indeed, I have

placed the numbers contained in the first column multiplied by 4·5. These results completely dissipate the suspicion that the effects observed with the polished tube could be due to a change of the reflecting power of its inner surface through the contact of that surface with the vapours employed.

It is easy to show in a general way the absorption of radiant heat by the stronger vapours. An open tube will answer the purpose, when quantitative results are not sought. The tube may even be dispensed with, and the vapour discharged from a slit in the open air between the pile and the source. A few specimen results obtained in this rough way will suffice for illustration. Two cubes of boiling water were employed, and in the usual manner the needle was brought to zero. Dry air was then urged from a gas bag (a common bellows would answer the same purpose) through a U-tube containing fragments of glass, moistened with the liquid whose vapour was to be examined. The mixed air and vapour were discharged in the open air in front of the pile, and the extreme limit of the swing of the galvanometer needle was noted.

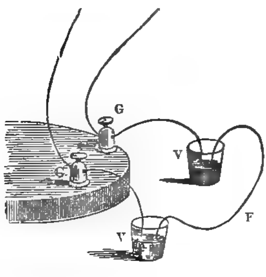
Vapour discharged in open air	Limit of swing of needle
Sulphuric ether	118°
Formic ether	117
Acetic ether	92
Amylene	91
Bisulphide of carbon	61
Valeric ether	32
Benzol	31
Alcohol	31

The influence of volatility here forces itself upon the attention. The action of course depends on the amount of vapour discharged, a quantity directly dependent on the volatility of the liquid. It is in consequence of its greater volatility that bisulphide of carbon is here able to transcend the far more energetic alcohol.

APPENDIX TO LECTURE XII.

I GIVE here a brief indication of the method of calibrating the galvanometer recommended by Melloni:—

FIG. 103.



Two small vessels, V V, half-filled with mercury, are connected by two short wires, with the extremities G G of the galvanometer. If, by means of a wire F, a communication be established between the two vessels, the quantity of electricity circulating in the galvanometer will be thus diminished, and with it the deflection of the needle.

Supposing that by varying the quantity of heat falling on the pile we obtain a series of deflections by the whole current of

4°, 8°, 12°, 16°, 20°, 24°;

and that when the branch wire is introduced, these deflections fall to

1°, 2°, 3°, 4°, 5°, 6°;

then, the whole currents are to the reduced currents, respectively, as 4 : 1. This *ratio of the currents* is maintained; but the *ratio of the deflections* is not. If, for example, the reduced current produce a deflection of 12°, the whole current will not produce a deflection of $12° \times 4 = 48°$, but only of 41°. If the reduced current produce a deflection of 16°, the deflection by the whole current will not be 64°, but 46° If 20° be the deflection of the reduced current, that of the whole current will not be 80°, but 50°. If 24° be the smaller deflection, the larger one will not be 96°, but only 53°. Making one of the lower degrees our unit, we should have, in the cases here considered, the following relations:—

Deflections . .	1°, 5°, 10°, 15°, 20°, 25° . . . . 41°, 46°, 50°, 53°.
Units . . .	1, 5, 10, 15, 20, 25 . . . . 48, 64 80, 96.

LECTURE XIII.

ACTION OF ODOROUS SUBSTANCES UPON RADIANT HEAT—ACTION OF OZONE UPON RADIANT HEAT—DETERMINATION OF THE RADIATION AND ABSORPTION OF GASES AND VAPOURS WITHOUT ANY SOURCE OF HEAT EXTERNAL TO THE GASEOUS BODY—DYNAMIC RADIATION AND ABSORPTION—RADIATION THROUGH THE EARTH'S ATMOSPHERE—INFLUENCE OF THE AQUEOUS VAPOUR OF THE ATMOSPHERE ON RADIANT HEAT—CONNECTION OF THE RADIANT AND ABSORBENT POWER OF AQUEOUS VAPOUR WITH METEOROLOGICAL PHENOMENA.

APPENDIX:—FURTHER DETAILS OF THE ACTION OF HUMID AIR.

ACTION OF PERFUMES ON RADIANT HEAT.

SCENTS and effluvia generally have long occupied the attention of observant men, and they have formed favourite illustrations of the 'divisibility of matter.' No chemist ever weighed the perfume of a rose; but in radiant heat we have a test more refined than the chemist's balance. Let us apply this test to odours, and see whether they, notwithstanding their almost infinite attenuation, do not, like the vapours, exercise a measurable influence on radiant heat.

We will operate in a very simple way. A number of small and equal squares of bibulous paper are rolled up so as to form little cylinders, each about two inches in length. Each paper cylinder is then moistened by dipping one end of it into an aromatic oil; the oil creeps by capillary attraction through the paper, until the whole of the roll becomes moist. The roll is introduced into a glass

tube, of a diameter which enables the paper cylinder to fill it without being squeezed, and between the drying apparatus and the exhausted experimental tube is placed the tube containing the scented paper. By the turning of a cock, dry air is drawn gently through the folds of the saturated paper. The air, laden with the perfume, passes forward into the experimental tube. The heat absorbed by one atmosphere of dry air we assume to be unity; and any additional absorption which these experiments reveal must be due to the scent which accompanies the air.

The following table gives a condensed view of the absorption of the substances mentioned in it, with reference to the unit just mentioned.

Perfumes.

Name of perfume	Absorption
Patchouli	30
Sandal-wood	32
Geranium	33
Oil of cloves	34
Otto of roses	37
Bergamot	44
Neroli	47
Lavender	60
Lemon	65
Portugal	67
Thyme	68
Rosemary	74
Oil of laurel	80
Camomile flowers	87
Cassia	109
Spikenard	355
Aniseed	372

The number of atoms of air here in the tube, in comparison with the molecules of the odours, must be regarded as almost infinite; still the latter, thinly scattered as they are, intercept from 30 to 372 times the quantity of radiant heat absorbed by the air. It would be idle to speculate

on the quantities of matter implicated in these results. Probably they would have to be multiplied by millions to bring them up to the pressure of the atmosphere.

In addition to these experiments on the essential oils, others were made on aromatic herbs. A number of such were obtained from Covent Garden Market. They were dry, in the common acceptation of the term; that is to say, they were not green, but withered. Still, I fear the results obtained with them cannot be regarded as faultless, on account of the probable admixture of aqueous vapour. The aromatic parts of the plants were stuffed into a glass tube eighteen inches long and a quarter of an inch in diameter. Previous to connecting them with the experimental tube, they were attached to a second air-pump, and dry air was carried over them for some minutes. They were then connected with the experimental cylinder, and treated as the essential oils; the only difference being that a length of eighteen inches, instead of two, was occupied by the herbs.

Thyme, thus examined, produced an absorption thirty-three times; peppermint thirty-four times; spearmint thirty-eight times; lavender thirty-two times; wormwood forty-one times; cinnamon fifty-three times that of the air which carried the scent. As already hinted, these results may be complicated with the action of aqueous vapour: its quantity, however, must have been infinitesimal.

ACTION OF OZONE.

There is another substance of great interest to the chemist, to which we may apply the test of radiant heat; but the attainable quantities of it are so minute as almost to elude measurement. I mean that extraordinary sub-

stance, ozone. This body is known to be liberated at the oxygen electrode, when water is decomposed by an electric current. To investigate its action, three different decomposing cells were constructed. In the first (No. 1) the platinum plates used as electrodes had each about four square inches of surface; the plates of the second (No. 2) had two square inches of surface; while the plates of the third (No. 3) had only one square inch of surface.

My reason for using electrodes of different sizes was this:—On first applying radiant heat to the examination of ozone, I constructed a decomposing cell, in which, to diminish the resistance of the current, very large platinum plates were used. The oxygen thus obtained, which ought to have embraced the ozone, showed scarcely any of the reactions of this substance. It hardly discoloured iodide of potassium, and was almost without action on radiant heat. A second decomposing apparatus, with smaller plates, was tried, and here the action, both on iodide of potassium and on radiant heat, was very decided. Being unable to refer these differences to any other cause than the different magnitudes of the plates, I formally attacked the subject, by operating with the three cells above described. Calling the action of the main body of the electrolytic oxygen unity; that of the ozone which accompanied it, in the respective cases, is given in the following table:

Number of cell	Absorption
No. 1	20
No. 2	34
No. 3	47

Thus, the modicum of ozone which accompanied the oxygen, and in comparison to which it is a vanishing quantity, exerted an action from twenty to forty-seven times that of the oxygen. The influence of the size

of the plates, or, in other words, of the *density* of the current where it enters the liquid, on the production of ozone is rendered strikingly manifest by these experiments.

Portions of the plates of cell No. 2 were next cut away, so as to make them smaller than those of No. 3. The reduction of the plates was accompanied by an increase of the action upon radiant heat; the absorption rose at once from 34 to

65.

The plates of No. 3 were next reduced, so as to make them smallest of all. The ozone now generated by No. 3 effected an absorption of

85.

Thus, we see that the action upon radiant heat advances as the size of the electrodes is diminished.

Heat is known to be very destructive of ozone; and suspecting the development of heat at the small electrodes of the cell last made use of, I surrounded the cell with a mixture of pounded ice and salt. Kept thus cool, the absorption of the ozone generated rose to

136.

There is a perfect correspondence between these results and those of MM. de la Rive, Soret, and Meidinger, though there is no resemblance between the modes of experiment. Such an agreement is calculated to augment our confidence in radiant heat, as an investigator of molecular condition.[1]

[1] M Meidinger commences his paper by showing the absence of agreement between theory and experiment in the decomposition of water, the difference showing itself very decidedly in a deficiency of oxygen *when the current was strong*. On heating his electrolyte, he found that this difference disappeared, the proper quantity of oxygen being then liberated. He at once surmised that the defect of oxygen might be due to the forma-

The quantities of ozone involved in the foregoing experiments would be perfectly unmeasurable by ordinary means. Still, its action upon radiant heat is so energetic as to place it beside olefiant gas, or boracic ether, as an absorbent—bulk for bulk, it might transcend either. No *elementary gas* that I have examined behaves at all like ozone. If it be oxygen, the oxygen atoms must be packed in groups. I sought to decide the question whether it is oxygen, or peroxide of hydrogen, in the following way. Heat destroys ozone. If it were oxygen only, heat would convert it into the common gas: if it were the hydrogen compound which some chemists consider it to be, heat would convert it into oxygen, plus aqueous vapour. The gas alone, admitted into the experimental tube, would give the neutral action of oxygen, but the gas, plus the aqueous vapour, would probably give a greater action. The dried electrolytic gas was first caused to pass through

tion of ozone; but how did the substance act to produce the diminution of the oxygen? If the defect were due to the great density of the ozone, the destruction of this substance, by heat, would restore the oxygen to its true volume. Strong heating, however, which destroyed the ozone, produced no alteration of volume, hence M. Meidinger concluded that the effect which he observed was not due to the ozone which remained mixed with the oxygen itself. He finally concluded, and justified his conclusion by satisfactory experiments, that the loss of oxygen was due to the formation, in the water, of peroxide of hydrogen by the ozone; the oxygen being thus withdrawn from the tube to which it belonged. He also, as M. de la Rive had previously done, experimented with electrodes of different sizes, and found the loss of oxygen much more considerable with a small electrode than with a large one; whence he inferred that the formation of ozone was facilitated by *augmenting the density of the current at the place where electrode and electrolyte meet.* The same conclusion is deduced from the above experiments on radiant heat. No two things could be more diverse than the two modes of proceeding. M. Meidinger sought for the oxygen which had disappeared, and found it in the liquid; I examined the oxygen actually liberated, and found that the ozone mixed with it augments in quantity, as the electrodes diminish in size. It may be added, that since the perusal of M. Meidinger's paper I have repeated his experiments with my own decomposition cells, and found that those which gave me the greatest absorption, also showed the greatest deficiency in the amount of oxygen liberated.

a glass tube heated to redness, and thence, without drying, direct into the experimental tube. Secondly, after heating, it was dried before entering the experimental tube. Hitherto I have not been able to establish, with certainty, a difference between the dried and undried gas. If, therefore, the act of heating develope aqueous vapour, the experimental means employed have not yet enabled me to detect it. For the present, therefore, I hold the belief, that ozone is produced by the packing of the atoms of elementary oxygen into groups; and that heat dissolves the bond of union between the atoms, thus disqualifying them for either intercepting or generating the motion, which, as molecules, they are competent to intercept and generate.[1]

DYNAMIC RADIATION AND ABSORPTION.

Your attention is now to be directed to a series of facts which surprised and perplexed me, when they were first observed. I permitted, on one occasion, a quantity of alcohol vapour, sufficient to depress the mercury gauge 0·5 of an inch, to enter the experimental tube; it produced a deflection of 72°. While the needle pointed to this high figure, and before pumping out the vapour, I allowed dry air to stream into the tube, and happened, as it entered, to keep my eye upon the galvanometer.

The needle, to my astonishment, sank speedily to zero, and went to 25° on the opposite side. The entry of the ineffective air not only neutralised the absorption previously observed, but left a considerable balance in favour of the

[1] The foregoing conclusion regarding the constitution of ozone was first stated at a time when the most eminent authorities regarded ozone as consisting of single atoms, and ordinary oxygen of groups of atoms. Chemical investigations have since independently established the view suggested by the above experiments on radiant heat.

face of the pile turned towards the source. A repetition of the experiment brought the needle down from 70° to zero, and sent it to 38° on the opposite side. In like manner, a very small quantity of the vapour of sulphuric ether produced a deflection of 30°; on allowing dry air to fill the tube, the needle descended speedily to zero, and swung to 60° at the opposite side.

My first thought, on observing these extraordinary effects, was, that the vapours had deposited themselves in opaque films on the plates of rock-salt, and that the dry air, on entering, had cleared these films away, and allowed the heat from the source free transmission.

But a moment's reflection dissipated this supposition. The clearing away of such a film could, at best, but restore the state of things existing prior to the entrance of the vapour. It might be conceived able to bring the needle again to 0°, but it could not possibly produce the negative deflection which, with ether vapour, reached the great amplitude of 60°. Nevertheless, I dismounted the tube, and subjected the plates of salt to a searching examination. No such deposit as that surmised was observed. The salt remained perfectly transparent while in contact with the vapour. How, then, are the effects to be accounted for?

Some of the experiments recorded in the Bakerian Lecture for 1860 had taught me that the dynamic heating of the air when it entered the exhausted tube was sufficient to produce a very sensible radiation on the part of any powerful vapour contained within the tube, though I was slow to believe that the enormous effect now under consideration could be thus accounted for. My first care was to determine the difference between the temperature of the experimental tube at the end furthest from the source of heat, and the air without. I then examined, by an extremely sensitive thermometer, the increase of temperature

produced by the admission of dry air into the tube, and the decrease of temperature consequent on pumping out, and found the former to be a considerable fraction of the total heat transmitted from the source. Could it be that the heat thus imparted to the alcohol and ether vapours, and radiated by them against the adjacent face of the pile, was more than sufficient to make good the loss by absorption? The *experimentum crucis* at once suggested itself. If the effects observed were due to the dynamic heating of the air, we ought to obtain them even when the sources of heat are entirely abolished. We should thus arrive at the solution of the novel, and at first sight utterly paradoxical, problem :—

To determine the radiation and absorption of a gas or vapour without any special source of heat external to the gas or vapour itself.

For this purpose we mount our glass tube, which is stopped at one end by a plate of glass, for we do not now need the passage of the heat through this end; and at the other end by a plate of rock-salt. In front of the salt is placed the thermo-pile, connected with its galvanometer. Though there is now no special source of heat acting upon the pile, the needle does not come quite to zero. Indeed the walls of this room, and the people who sit around, are so many sources of heat, to neutralise which, and thus to bring the needle accurately to zero, I must slightly warm the defective face of the pile. This is done without any difficulty by a large Leslie's cube of lukewarm water, placed at a distance; the needle is now at zero.

The experimental tube being exhausted, air is permitted to enter. This air is warmed dynamically; and if its atoms possessed any sensible power of communicating their motion to the luminiferous ether, we should have, from each atom, a train of waves impinging on the face of the pile. But you observe scarcely any mo-

tion of the galvanometer, and hence we may infer that the quantity of heat radiated by the air is exceedingly small. The deflection produced is 7°.

But are these 7° really due to the radiation of the air? To answer this question I open one of the ends of the experimental tube, and place a bit of black paper as a lining within it, thus covering the interior surface of the tube for a length of 12 inches. On exhausting the tube and permitting air to enter as in the last experiment, the needle flies through an arc of 70°. The paper lining, warmed by the air, radiates against the pile in this copious way. Now the interior surface of the tube itself must do the same, though in a less degree, and to this surface, and not to the air itself, the deflection of 7° which we have just obtained is, I believe, to be ascribed.

Removing the lining from the tube, instead of air we will permit nitrous oxide to stream into it; the needle swings to 28°, thus showing the superior radiative power of this gas over that of air. On working the pump, the gas within the experimental tube is chilled. Into it the pile pours its heat, a swing of 20° in the opposite direction being the consequence. The superior absorptive power of this gas over air is thus demonstrated.

We have already established by means of a special external source of heat that olefiant gas is highly gifted with the power of absorption and radiation. Tested by this new method, on permitting it to enter the exhausted tube, the needle swings through an arc of 67°. Let it waste its heat, and let the needle come to zero. On pumping out, the chilling of the gas within the tube produces a deflection of 40° on the side of cold. We have certainly here a key to the solution of the enigmatical effects, observed with the alcohol and ether vapour.

For the sake of convenience we may call the heating

of the gas on entering the vacuum *dynamic heating*; its radiation may be called *dynamic radiation*, and its absorption, when it is chilled by pumping out, *dynamic absorption*. These terms being understood, the following table explains itself. In each case, the extreme limit to which the needle swung, on the entry of the gas into the experimental tube, is recorded.

DYNAMIC RADIATION OF GASES.

Name	Limit of swing
Air	7°
Oxygen	7
Hydrogen	7
Nitrogen	7
Carbonic oxide	19
Carbonic acid	21
Nitrous oxide	31
Olefiant gas	63

We observe that the order of the radiative powers, determined in this novel way, is the same as that already obtained from a totally different mode of experiment. It must be borne in mind that the discovery of dynamic radiation is comparatively recent, and that the conditions of perfect accuracy have not yet been developed; it is, however, certain, that the mode of experiment is susceptible of the highest degree of precision.

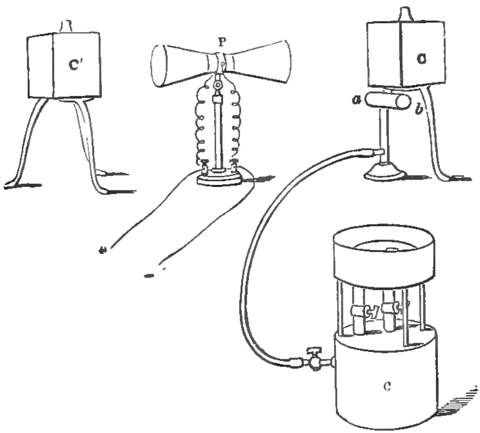
Let us now turn to our vapours, and while dealing with them I shall endeavour to unite two effects which, at first sight, might appear utterly incongruous. We have already learned that a polished metal surface emits an extremely feeble radiation; but that, when the same surface is coated with varnish, the radiation is copious. In the communication of motion to the ether of space,[1] the atoms

[1] If we could change either the name given to the interstellar mediums or that given to certain volatile liquids by chemists, it would be an advantage. It is difficult to avoid confusion in the use of the same term for objects so utterly diverse.

of the metal need a mediator, and this they find in the varnish. I have now to prove to you that a metallic surface may be varnished by a gas.

The arrangement before you enables me to to cause a thin stream of olefiant gas to pass from the gasholder G (fig. 104) through a slit tube a b, over the heated surface of the cube C. At present no gas issues, and the radiation from C against the thermo-pile P is neutralised by that from C′. I now pour gas from G over the cube C; and though the

FIG. 104.



surface is actually cooled by the passage of the gas, for the gas has to be warmed by the metal, the radiation is considerably augmented. As soon as the gas begins to flow, the needle begins to move, and reaches an amplitude of 45°.

We have here varnished a metal by a gas, but a more interesting and subtle effect is the varnishing of one gaseous body by another. I attach a flask containing acetic ether to the experimental tube, and permit the vapour to enter it, until the mercury column has been depressed half an inch. This vapour is to be our varnish.

Permitting dry air to enter the tube, the air is dynamically heated, but we know its incompetence to radiate its heat. Now however it comes into contact with the acetic ether vapour, and communicating its heat-motion to the vapour, the latter is able to send the motion on to the pile. The needle swings through an arc of 70° by the radiation from the vapour molecules. It is not necessary to insist upon the fact, that in this experiment the vapour bears precisely the same relation to the air as the varnish to the gold, silver and copper employed in our former experiments.

We permit the vapour to pour away the heat: it is the discharger of the calorific motion generated by the moving air. The needle returns to zero. On working the pump, the air, and through it the vapour within the tube, is chilled, the needle moving to nearly 45° on the other side of zero. In this way, the dynamic radiation and absorption of the vapours mentioned in the following table have been determined; air being the substance employed to heat the vapour. The limit of the first swing of the needle is noted as before.

DYNAMIC RADIATION AND ABSORPTION OF VAPOURS.

	Deflections	
	Radiation	Absorption
1. Bisulphide of carbon	14°	6°
2. Iodide of methyl	20	8
3. Benzol	30	14
4. Iodide of ethyl	34	16
5. Methylic alcohol	36	18
6. Chloride of amyl	41	23
7. Amylene	48	26
8. Alcohol	50	28
9. Sulphuric ether	64	34
10. Formic ether	69	38
11. Acetic ether	70	43

We have here used eleven different kinds of vapour, as varnish for the air, and we find that the dynamic radiation and absorption augment exactly in the order established by experiments with external sources of heat. We also see how strictly dynamic radiation and absorption go hand in hand, the one augmenting and diminishing with the other.

A reflection here presents itself, which is worthy of our consideration. We have measured the dynamic radiation of olefiant gas, by allowing the gas to enter our tube, until the latter was quite filled. It is manifest that those portions of the warmed column of gas most distant from the pile must radiate *through the gas in front of them*, and, in this forward portion of the column, a large quantity of the rays emitted by its hinder portion will be absorbed. In fact, it is quite certain that if we made our column sufficiently long, the frontal portions would act as a perfectly impenetrable screen to the radiation from the hinder ones. If this reasoning be correct, then, by cutting off the part of the gaseous column most distant from the pile, we should diminish only in a slight degree the radiation reaching the pile.

In the case, on the contrary, of a vapour, where the pressure is only 0·5 of an inch, the radiating molecules are much wider apart than in the case of the olefiant gas; consequently the radiation of the hinder portions of the column of vapour will have a comparatively open door, through which to reach the pile. These considerations render it manifest that, in the case of the vapour, a greater length of column is available for radiation than in the case of the gas. This leads to the further conclusion, that if we shorten our column, we shall diminish the radiation, in the case of the vapour, more considerably than in that

of the gas. Let us now bring this reasoning to the test of experiment.

We have already found the dynamic radiation of the following four substances, when the radiating column was 2 feet 9 inches long, to be represented by the annexed deflections:—

Olefiant gas (1 atmosphere)			63
Sulphuric ether vapour (0·5 inch)			64
Formic ether	„	„	69
Acetic ether	„	„	70

olefiant gas giving the least dynamic radiation.

Experiments made, in precisely the same manner, with a tube 3 inches long, or $\frac{1}{11}$th of the former length, gave the following deflections:—

Olefiant gas		39°
Sulphuric ether vapour		11
Formic ether	„	12
Acetic ether	„	15

The verification of our reasoning is therefore complete. It is proved that in the long tube the dynamic radiation of the vapour exceeds that of the gas, while in a short one the dynamic radiation of the gas greatly exceeds that of the vapour. The result proves, if proof were needed, that though diffused in air, the vapour molecules are really the centres of radiation.

AQUEOUS VAPOUR.

Up to the present point, I have purposely omitted all reference to the most important vapour of all, as far as our world is concerned—the vapour of water. This vapour, as you know, is always diffused through the atmosphere. The clearest day is not exempt from it: indeed, in the Alps, the purest skies are often the most treacherous, the firmamental blue deepening with the amount of aqueous

vapour in the air. It is needless, therefore, to remind you that when aqueous vapour is spoken of, nothing visible is meant. It is not fog; nor is it cloud or mist of any kind. These are formed of vapour which has been condensed to water; but the true vapour, with which we have to deal, is an impalpable transparent gas. It is diffused everywhere thoughout the atmosphere, though in very different proportions.

To prove the existence of aqueous vapour in the air by which we are now surrounded, a copper vessel, filled an hour ago with a mixture of pounded ice and salt, is placed in front of you. The surface of the vessel was then black, but it is now white—furred all over with hoar-frost. This has been produced by the condensation, and subsequent congelation upon its surface, of the aqueous vapour of this room. The white substance can be scraped off in sufficient quantity to form a small snowball. On the plate of glass used to cover the vessel, the vapour is not congealed, but it is condensed so copiously that when the plate is held edgeways, the water runs off it in a stream.

The quantity of this vapour is really small. Oxygen and nitrogen constitute about $99\frac{1}{2}$ per cent. of our atmosphere, and of the remaining 0·5, about $0{\cdot}4\frac{1}{2}$ is aqueous vapour. The rest is carbonic acid. Had we not been already acquainted with the action of almost infinitesimal quantities of matter on radiant heat, we might well despair of being able to establish a measurable absorption by the aqueous vapour of our atmosphere. Indeed I quite neglected this substance for a time, and could hardly credit my first result, which made the action of the aqueous vapour of our laboratory fifteen times that of the air in which it was diffused. This, however, by no means expresses the true relation between aqueous vapour and dry air.

To illustrate this point, our first arrangement (shown

in the frontispiece) has been resumed. It consists, you know, of a brass tube s, s′, and two sources of heat c, c′, acting on the opposite faces of the pile P. The experiment with dry air being repeated, the needle does not move sensibly. If close to it, you would observe a motion through about one degree. Could we get our air absolutely pure, its action would be even less than this. Making the same experiment with the undried air of this room, the needle moves as the air enters, the final deflection being 48°. The needle will point steadily to this figure, as long as the sources of heat remain constant, and as long as the air continues in the tube. These 48° correspond to an absorption of 72; that is to say, the aqueous vapour contained in the atmosphere of this room to-day, absorbs 72 times the amount of radiant heat absorbed by the air itself.

This result is obtained with perfect ease, but not without due care. In comparing dry with humid air, it is perfectly essential that both substances be pure. You may work for months with an imperfect drying apparatus, and fail to obtain air which shows this almost total absence of action on radiant heat. An amount of organic impurity, too small to be seen by the eye, is sufficient to augment fiftyfold the action of the air. You are now better prepared for such facts than I was, when they first forced themselves on my attention. The experimental result which we have just arrived at, will, if true, have so important an influence on the science of meteorology, that it ought to be subjected to the closest scrutiny. First of all, then, look at this piece of rock-salt brought in from the next room, where it has stood for some time near a tank, but not in contact with visible moisture. The salt is wet; it is a hygroscopic substance, and freely condenses moisture upon its surface. Here, also, is a polished plate of the substance, which is now quite dry: I breathe upon

it, and instantly its affinity for moisture causes the vapour of my breath to overspread the surface, in a film which exhibits beautifully the colours of thin plates.[1] Now we know, from Melloni's table (page 310) how opaque a solution of rock-salt is to the calorific rays, and hence arises the question whether, in the above experiment with undried air, we may not in reality be measuring the action of a thin stratum of such a solution, deposited on our plates of salt, instead of the pure action of the aqueous vapour of the air.

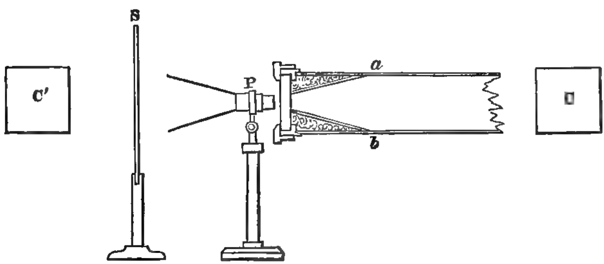
If we operate incautiously, and, more particularly, if it be our actual intention to wet the plates of salt, we may readily obtain the deposition of moisture. This is a point on which any competent experimenter will soon instruct himself; but the essence of good experimenting consists in the exclusion of circumstances which would render the pure and simple questions which we intend to put to Nature, impure and composite ones. The first way of replying to the doubt here raised is to examine our plates of salt; if the experiments have been properly conducted, no trace of moisture is found upon the surface. A thousand experiments might be cited in proof of this. To render success more certain, however, we will slightly alter the arrangement of our apparatus. Hitherto we have had the thermo-electric pile and its two reflectors entirely *outside* the experimental cylinder. I now detach one of the reflectors from the pile and push it into the experimental cylinder. The hollow reflecting cone is 'sprung' at its base *a b* (fig. 105), so that it is held tightly by its own pressure against the inner surface of the cylin-

[1] Receiving the beam from the electric lamp upon the polished plate of salt, so as to reflect the light on to a screen, and placing a lens in front of the salt, so as to produce an image of its polished surface on the screen; on breathing against the salt through a glass tube, rings of vivid iridescence instantly flash forth.

der. The space between the outer surface of the reflector and the inner surface of the tube is filled with fragments of fused chloride of calcium. Against the inner surface of the rock-salt plate, the narrow end of the reflector now abuts. Bringing the face of the pile P close up to the plate, though not into actual contact with it, our arrangement is complete.

In the first place, it is to be remarked, that the plate of salt nearest to the source of heat is never moistened, unless the experiments are of the coarsest character. Its

FIG. 105.

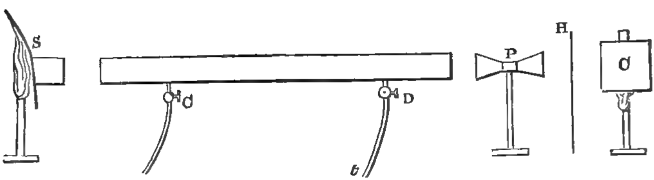


proximity to the source enables the heat to chase away every trace of humidity from the surface of that plate. The distant plate is the one in danger, and now we have the circumferential portions of this plate kept perfectly dry by the chloride of calcium. No moist air can at all reach the rim of the plate; while upon its central portion, measuring about a square inch in area, *we have converged our entire radiation.* On *à priori* grounds, we should conclude that it is quite impossible for a film of moisture to collect there; and this conclusion is justified by fact. Testing, as before, the dried and the undried air of this room, we find, as in the former instance, that the latter produces seventy times the effect of the former. On examining the plate, even with a lens, not the slightest trace of moisture

is found upon its surface. It was carefully polished when attached to the tube; it is as perfectly polished now. Glass, or rock-crystal, could not show a surface more exempt from any appearance of moisture. This experiment is conclusive against the hypothesis that the effects observed are due to a film of brine, instead of to aqueous vapour.

Further, we may do away entirely with the plates of salt and obtain substantially the same effect in a tube *open at both ends.* Here, as in other cases, the practical tact of the experimenter must come into play. The source on the one hand and the pile on the other being freely exposed to the air, a very slight agitation acting upon either, would disturb, and might indeed altogether mask, the effect we seek. The air, therefore, must be introduced into the open tube, without producing any commotion, either near the source or near the pile. The length of the experimental tube here employed is 4 feet 3 inches; at C (fig. 106) is

FIG. 106.



a cock connected with an india-rubber bag containing common air, and subjected by a weight to gentle pressure; at D is a second cock, connected by a flexible tube, *t*, with an air-pump. Between the cock C, and the india-rubber bag, drying tubes are introduced; and when that cock is opened, the air is forced gently through the drying tubes into the experimental cylinder. The air-pump is slowly worked at the same time, the dry air being thereby drawn towards D. The distance of C from the source S is 18 inches, and the distance of D from the pile P is 12

inches: the compensating cube C, and the screen H, serve the same purposes as before. By thus isolating the central portion of the tube, we can displace dry air by moist, or moist air by dry, without permitting any agitation to reach either the source or the pile.

Suppose the tube filled with the common air of the laboratory, the needle of the galvanometer pointing to zero. Air is permitted to pass through the drying apparatus, and to enter the open tube at C, the pump being worked as already described. When the dry air enters, the needle begins to move, the substitution of dry air for the air of the laboratory rendering the medium more transparent to the rays of heat. The final deflection thus obtained is 45°, where the needle steadily remains.

When the supply of dry air is cut off, and the pump ceases to work, the needle sinks, but with great slowness, indicating a correspondingly slow diffusion of the aqueous vapour of the adjacent air into the dry air of the tube. If the pump be worked, the removal of the dry air is hastened, and the needle sinks more speedily to zero. The experiment may be made a hundred times in succession without any deviation from this result; on the entrance of the dry air, the needle invariably goes up to 45°, showing augmented transparency; on the entrance of the undried air, the needle sinks to 0°, showing augmented absorption.

Here, then, we have substantially the same result as that obtained when the experimental tube was stopped with plates of rock-salt. The action, therefore, cannot be referred to a film of moisture deposited upon the surface of the plates. And be it remarked that there is not the slightest caprice or uncertainty in these experiments when properly conducted. They have been executed at different times and seasons; the tube has been dismounted and remounted; the suggestions of eminent men who have seen the experi-

ments, and whose object it was to test the results, have been complied with; but no deviation from the effects just recorded has been observed. The entrance of each kind of air is invariably accompanied by its characteristic action; the needle is under the most complete control: in short, no experiments hitherto made with solid and liquid bodies are more certain in their execution than the foregoing experiments on dry and humid air.

Introducing a tin screen between the experimental cylinder and the pile, one of the sources of heat is entirely shut off. The deflection produced by the other source indicates the total radiation. This deflection corresponds to about 780 of the units which have been hitherto adopted; one unit being the quantity of heat necessary to move the needle from 0° to 1°. The deflection of 45° corresponds to 62 units; out of 780, therefore, 62 have been absorbed by the moist air. The following statement gives us the absorption per hundred:—

$$780 : 100 = 62 : 7{\cdot}9.$$

An absorption of nearly 8 per cent. was, therefore, effected by the atmospheric vapour which occupied the tube between C and D. Air *perfectly saturated* gives a still greater absorption.

This absorption took place, notwithstanding the partial sifting of the heat, in its passage from the source to C, and from D to the pile. The moist air, moreover, was probably only in part displaced by the dry. In other experiments with a tube 4 feet long, and polished within, it was found that the atmospheric vapour, on a day of average dryness, absorbed over 10 per cent. of the radiation from our source. Regarding the earth as a source of heat, I estimate that at least 10 per cent. of its heat is intercepted within ten feet of the surface. This

single fact suggests the enormous influence which this newly-developed property of aqueous vapour must have in the phenomena of meteorology.

But we have not yet disposed of all objections, which in reference to this subject have been of the most Protean character. It has been intimated to me that the air of our laboratory might be impure; the suspended carbon particles of the London air have also been referred to, as a possible cause of the absorption ascribed to aqueous vapour. The same results however were obtained with air brought in impervious bags from Hyde Park, Primrose Hill, Hampstead Heath, Epsom Downs, and from various parts of the Isle of Wight. *The aqueous vapour of the air from all these localities, examined in the usual way, exerted an absorption seventy times that of the air in which the vapour was diffused.*

Again. The air of the laboratory was dried and purified, until its absorption fell below unity; this purified air was then led through a U-tube, filled with fragments of perfectly clean glass moistened with distilled water. Its neutrality, when dry, showed that all prejudicial substances had been removed from it, and in passing through the U-tube, it could take up nothing but the pure vapour of water. The vapour thus carried into the experimental tube produced an action ninety times greater than that of the air which carried it.

The tube with which these experiments were made is polished within, and it has been surmised that the vapour of the humid air had, on entering, deposited itself upon the interior surface of the tube, thus diminishing its reflective power, and producing an effect apparently the same as absorption. The doubt is not capable of being sustained. The amount of heat intercepted is accurately proportional to the quantity of air present. This is shown by the following table, which gives the absorption,

by humid air, at pressures varying from 5 to 30 inches of mercury:—

HUMID AIR.

Pressure in inches	Absorption Observed	Absorption Calculated
5	16	16
10	32	32
15	49	48
20	64	64
25	82	80
30	98	96

The third column of this table is calculated on the assumption that the absorption is proportional to the quantity of vapour in the tube, and the agreement of the calculated and observed results shows this to be the case, within the limits of the experiment. It cannot be supposed that effects so regular as these, and agreeing so completely with those obtained with small quantities of other vapours, and even with small quantities of the permanent gases, can be due to the condensation of the vapour on the interior surface. When, moreover, five inches of air were in the tube, less than one-sixth of the vapour necessary to saturate the space was present. The driest day would make no approach to this dryness. That condensation which should destroy, by its action upon the inner reflector, quantities of heat so accurately proportional to the quantities of matter present, should here occur is not to be thought of.

BEARINGS ON METEOROLOGY.

Some remarkable corroborations of these views have been published by that excellent meteorologist. Gen Richard Strachey, of the Royal Engineers. And his tes-

timony is rendered all the more valuable by the fact that it is based on observations made long before the property of aqueous vapour here developed was known to have an existence. From his important paper, published in the Philosophical Magazine for July, 1866, I extract a single representative series of observations, made between the 4th and the 25th of March, 1850; during which period 'the sky remained remarkably clear, while great variations in the quantity of vapour took place.' The first column of figures gives the tension of aqueous vapour, and the second the fall of the thermometer from 6.40 p.m. to 5.40 a.m.

Tension of vapour	Fall of thermometer
0·888 inches	6·0°
0·849 „	7·1
0·805 „	8·3
0·749 „	8·5
0·708 „	10·3
0·659 „	12·6
0·605 „	12·1
0·554 „	13·1
0·435 „	16·5

The general result is here unmistakable. In clear nights the fall of the thermometer, which expresses the energy of the radiation, is determined by the amount of transparent aqueous vapour in the air. The presence of the vapour checks the loss, while its removal favours radiation and promotes the nocturnal chill. We shall subsequently add another powerful proof to those here adduced.

The aqueous vapour which absorbs heat thus greedily, radiates it copiously; and this fact must come powerfully into play in the tropics. We know that the sun raises from the equatorial ocean enormous quantities of vapour, and that immediately under him, in the region of calms, the rain,

due to the condensation of the vapour, descends in deluges. Hitherto, this has been ascribed to the chilling which accompanies the expansion of the ascending air; and no doubt this, as a true cause, must produce its proportionate effect. But the radiation from the vapour itself must also be influential. When a column of saturated air ascends from the equatorial ocean, the radiation from it is for some time intercepted, and in great part returned to it, by the surrounding vapour. But the quantity of vapour in the atmosphere diminishes rapidly as we ascend; the decrement of vapour-tension, as proved by Hooker, Strachey, and Welsh, is much more speedy than that of the air itself; and finally, our vaporous column finds itself elevated beyond the protecting screen which, during the first portion of its ascent, was spread above it. It is now in the presence of pure space, and into space it pours its heat, without stoppage or requital. To the loss of heat thus endured, the condensation of the vapour, and its torrential descent, must certainly be in part ascribed.

Similar remarks apply to the formation of cumuli in our own latitudes; they are the heads of vaporous columns which rise from the earth's surface, and are precipitated as soon as they reach a certain elevation. Thus, the visible cloud forms the capital of an invisible pillar of saturated air. The top of such a column, raised above the lower vapour-screen which clasps the earth, and offering itself to space, is chilled by radiation and precipitated as cloud.

Mountains act as condensers, partly by the coldness of their own masses; which they owe to their elevation. Above them spreads no vapour-screen of sufficient density to intercept their heat, which consequently passes unrequited into space. When the sun is withdrawn, this loss is shown by the quick descent of the thermometer.

The difference between a thermometer which, properly protected, gives the true temperature of the night air, and one which is permitted to radiate freely towards space, must be greater at high elevations than at low ones. This conclusion is confirmed by observation. On the Grand Plateau of Mont Blanc, for example, MM. Martins and Bravais found the difference between two such thermometers to be 24° Fahr.; when a difference of only 10° was observed at Chamouni.

A freedom of escape, similar to that from bodies of vapour at great elevations, would occur at the earth's surface generally, were the aqueous vapour removed from the air above it; for the great body of the atmosphere is a practical vacuum, as regards the transmission of radiant heat. The withdrawal of the sun from any region over which the atmosphere is dry, must be followed by quick refrigeration. The removal, for a single summer night, of the aqueous vapour from the atmosphere which covers England would be attended by the destruction of every plant which a freezing temperature could kill. The moon would be rendered entirely uninhabitable by beings like ourselves through the operation of this single cause. With a radiation uninterrupted by aqueous vapour, the difference between her monthly maxima and minima must be enormous. The winters of Thibet are almost unendurable. Witness how the isothermal lines dip from the north into Asia, in winter, as a proof of the low temperature of this region. Humboldt has dwelt upon the 'frigorific power' of the central portions of this continent, and controverted the idea that it was to be explained by reference to the elevation; there being vast expanses of country, not much above the sea-level, with an exceedingly low temperature. But not knowing the influence which we are now studying, Humboldt, I imagine, omitted the most potent cause of the cold. The

refrigeration at night is extreme because the air is dry. In Sahara, where 'the soil is fire and the wind is flame,' the cold at night is often painful to bear. In short, it may be safely predicted, that wherever the air is *dry*, the daily thermometric range will be great. This, however, is quite different from saying that where the air is *clear*, the thermometric range will be great. Great clearness to light is perfectly compatible with great opacity to heat; the atmosphere may be charged with aqueous vapour while a deep blue sky is overhead, and on such occasions the terrestrial radiation would, notwithstanding the 'clearness,' be intercepted.

The following remarkable passage from Hooker's 'Himalayan Journals,'[1] also bears upon the present subject: 'From a multitude of desultory observations I conclude that, at 7,400 feet, 125·7°, or 67° above the temperature of the air, is the average effect of the sun's rays on a black bulb thermometer. . . . These results, though greatly above those obtained at Calcutta, are not much, if at all, above what may be observed on the plains of India. The effect is much increased by elevation. At 10,000 feet, in December, at 9 a.m., I saw the mercury mount to 132°, while the temperature of shaded snow hard by was 22°. At 13,100 feet, in January, at 9 a.m., it has stood at 98°, with a difference of 68·2°, and at 10 a.m. at 114°, with a difference of 81·4°, *whilst the radiating thermometer on the snow had fallen at sunrise to* 0·7°.'

These enormous differences between the shaded and the unshaded air, and between the air and the snow, are, no doubt, due to the comparative absence of aqueous vapour at these elevations. The air is incompetent to check either the solar or the terrestrial radiation, and hence the maximum heat in the sun and the maximum

[1] First edition vol. ii. p. 407.

cold in the shade must stand very wide apart. The difference between Calcutta and the plains of India is accounted for in the same way.

Dr. Livingstone, in his 'Travels in South Africa,' has given some striking examples of the difference in nocturnal chilling when the air is dry and when it is laden with moisture. Thus he finds in South Central Africa during the month of June, 'the thermometer early in the mornings at from 42° to 52°; at noon, 94° to 96°,' or a mean difference of 48° between sunrise and midday. The range would probably have been found still greater had not the thermometer been placed in the shade of his tent, which was pitched under the thickest tree he could find. He adds, moreover, 'the sensation of cold after the heat of the day was very keen. The Balonda at this season never leave their fires till nine or ten in the morning. As the cold was so great here, it was probably frosty at Linyanti; I therefore feared to expose my young trees there.' [1]

Livingstone afterwards crossed the continent and reached the river Zambesi at the beginning of the year. Here the thermometric range was reduced from 48° to 12°. He thus describes the change he felt on entering the valley of the river: 'We were struck by the fact, that as soon as we came between the range of hills which flank the Zambesi, the rains felt warm. At sunrise the thermometer stood at from 82° to 86°: at midday, in the coolest shade, namely, in my little tent, under a shady tree, at 96° to 98°; and at sunset at 86°. This is different from anything we experienced in the interior.' [2]

Proceeding towards the mouth of the river on January 16 he makes the following additional observation: 'The Zambesi is very broad here (at Zumbo), but contains many inhabited islands. We slept opposite one on the 16th,

[1] *Livingstone's Travels*, p. 484. [2] Ibid. p. 575.

called Shibanga. The nights are warm, the temperature never falling below 80°; it was 91° even at sunset. One cannot cool the water by a wet towel round the vessel. . . .'[1]

In Central Australia the daily range of the thermometer is still greater. The following extract is from a paper by Prof. W. S. Jevons 'On some Data concerning the Climate of Australia and New Zealand': '. . . In the interior of the continent of Australia the fluctuations of temperature are immensely increased. The heat of the air, as described by Captain Sturt, is fearful during summer; thus, in about lat. 30° 50′ S., and lon. 141° 18′ E., he writes: 'The thermometer every day rose to 112° or 116° in the shade, and in the direct rays of the sun from 140° to 150°.' Again, 'at a quarter past three p.m. on January 21 (1845), the thermometer had risen to 131° in the shade, and to 154° in the direct rays of the sun.' . . . In the winter the thermometer was observed as low as 24°, giving an extreme range of 107°.

'The fluctuations of temperature were often very great and sudden, and were severely felt. On one occasion (October 25), the temperature rose to 110° during the day, but a squall coming on, it fell to 38° at the following sunrise; it thus varied 72° in less than twenty-four hours. . . . Mitchell, on his last journey to the N.W. interior, had very cold frosty nights. On May 22, the thermometer stood at 12° in the open air. . . . Still, in the daytime, the air was warm, and the daily range of temperature was enormous. Thus, on June 2, the thermometer rose from 11° at sunrise to 67° at four p.m.; or through a range of 56°. On June 12, the range was 53°, and on many other days nearly as great.'

Without quitting Europe, we find places where, while

[1] *Livingstone's Travels*, p. 589.

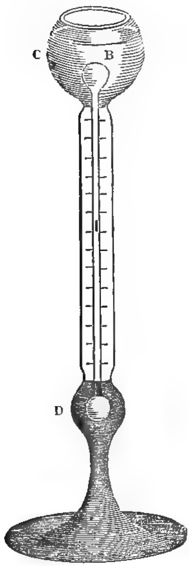
the day temperature is very high, the hour before sunrise is intensely cold. I have often experienced this in the Post-wagens of Germany; and I am informed that the Hungarian peasants, if exposed at night, take care, even in hot weather, to protect themselves by heavy cloaks against the nocturnal chill. The observations of MM. Bravais and Martins on the Grand Plateau of Mont Blanc have been already referred to. M. Martins has recently added to our knowledge by making observations on the heating of the soil at great elevations. He finds on the summit of the Pic du Midi the heat of the soil exposed to the sun, above that of the air, to be twice as great as in the valley at the base of the mountain. 'The immense heating of the soil,' writes M. Martins, 'compared with that of the air on high mountains, is the more remarkable since, during the nights, the cooling by radiation is there much greater than in the plain.' The observations of the Messrs. Schlagintweit furnish, if I mistake not, many illustrations of the action of aqueous vapour.

No doubt, I think, can be entertained, that the extraordinary energy of water as a radiant, in all its states of aggregation, must play a powerful part in a mountain region. As vapour, it pours its heat into space, and promotes condensation; as liquid, it pours its heat into space and promotes congelation; as snow, it pours its heat into space, and thus converts the surfaces on which it rests into more powerful condensers than they otherwise would be. Of the numerous wonderful properties of water, not the least important is the power which it possesses, of thus discharging the motion of heat upon the interstellar ether.

And here we are led to an easy explanation of a fact which evidently perplexed Sir John Leslie. This celebrated experimenter constructed an instrument which he named an *æthrioscope*, the function of which was to determine the radiation against the sky. It consisted of

two glass bulbs united by a vertical glass tube, so narrow that a little column of liquid was supported in the tube by its own adhesion. The lower bulb D (fig. 107) was protected by a metallic envelope, and gave the temperature of the air; the upper bulb B was blackened, and was surrounded by a metallic cup C, which protected the bulb from terrestrial radiation.

FIG. 107.



'This instrument,' says its inventor, 'exposed to the open air in clear weather, will at all times, both during the day and the night, indicate an impression of cold shot downwards from the higher regions. . . . The sensibility of the instrument is very striking, for the liquor incessantly falls and rises in the stem with every passing cloud. But the cause of its variations does not always appear so obvious. Under a fine blue sky the *æthrioscope* will sometimes indicate a cold of 50 millesimal degrees; yet on other days, *when the air seems equally bright*, the effect is hardly 30°.' This anomaly is simply due to the difference in the quantity of aqueous vapour present in the atmosphere. Indeed, Leslie himself connects the effect with aqueous vapour in these words: 'The pressure of hygrometric moisture in the air probably affects the instrument.' It is not, however, the 'pressure'[1] that is effective; it was the presence of invisible vapour that intercepted the radiation from the æthrioscope, while its absence opened a door for the escape of this radiation into space. As regards experiments on terrestrial radiation, a new definition will

[1] Possibly the word 'pressure' is a misprint for 'presence.'

have to be given of 'a clear day.' It is manifest, for example, that in experiments with the pyrheliometer,[1] two days of equal visual clearness may give totally different results. The radiation from the pyrheliometer is often intercepted, when no cloud is seen. Could we, however, make the constituents of the atmosphere, its vapour included, objects of vision, we should see sufficient to account for this result.

Another interesting point, on which this subject has a bearing, is the theory of *serein*. 'Most authors,' writes Melloni, 'attribute to the cold, resulting from the radiation of the air, the excessively fine rain which sometimes falls from a clear sky, during the fine season, a few moments after sunset.' 'But,' he continues, 'as no fact is yet known which directly proves the emissive power of pure and transparent elastic fluids,[2] it appears to me more conformable,' &c. &c. If the difficulty here urged against the theory of *serein* be its only one, the theory will stand; for transparent elastic fluids are now proved to possess the power of radiation which the theory assumes. It is not, however, to radiation from the *air* that the chilling is to be ascribed, but to radiation from the body itself, whose condensation produces the *serein*.

[1] The instrument is described in a subsequent lecture.

[2] This statement indicates the state of the science of thermotics in reference to the gaseous form of matter when these researches were begun.

LECTURE XIV.

ABSORPTION OF HEAT BY VOLATILE LIQUIDS—ABSORPTION OF HEAT BY THE VAPOURS OF THOSE LIQUIDS AT A COMMON PRESSURE—ABSORPTION OF HEAT BY THE SAME VAPOURS WHEN THE QUANTITIES OF VAPOUR ARE PROPORTIONAL TO THE QUANTITIES OF LIQUID—COMPARATIVE VIEW OF THE ACTION OF LIQUIDS AND THEIR VAPOURS UPON RADIANT HEAT—PHYSICAL CAUSE OF OPACITY AND TRANSPARENCY—INFLUENCE OF TEMPERATURE ON THE TRANSMISSION OF RADIANT HEAT—CHANGES OF POSITION THROUGH CHANGES OF TEMPERATURE—RADIATION FROM FLAMES—INFLUENCE OF OSCILLATING PERIOD ON THE TRANSMISSION OF RADIANT HEAT—EXPLANATION OF CERTAIN RESULTS OF MELLONI AND KNOBLAUCH.

THE natural philosophy of the future will in great part consist of inquiries into the relations subsisting between ordinary matter and the luminiferous ether. Regarding the constitution and the motion of the ether itself, the optical investigations of the last half-century leave little to be desired; but regarding the atoms and molecules, whence issue the undulations of light and heat, and their relations to the medium in which they are immersed, these investigations teach us little. To come closer to the origin of the ethereal waves—to obtain, if possible, some experimental hold of the oscillating atoms themselves—has been the main object of those researches on the radiation and absorption of heat by gases and vapours which, in brief outline, have been sketched before you.

When a gas is condensed to a liquid, the molecules approach and grapple with each other, by forces which are insensible as long as the gaseous state is maintained. But though thus condensed and enthralled, the all-pervading

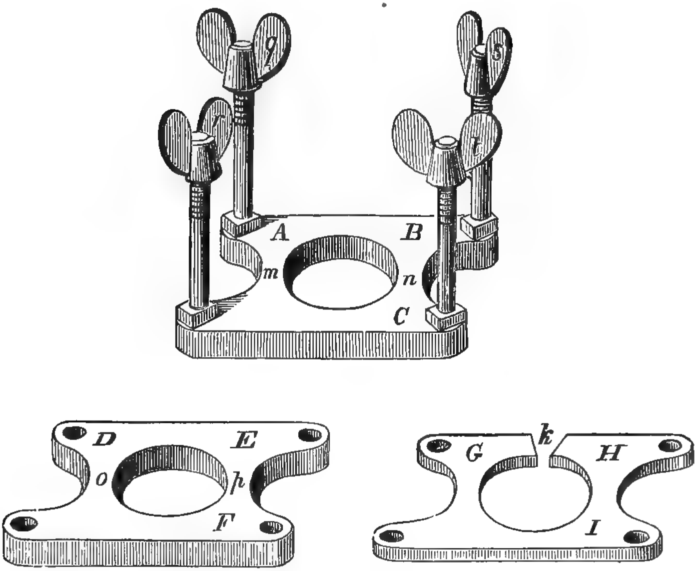
ether still surrounds the molecules. If, then, the power of radiation and absorption depend upon them individually, we may expect that the deportment towards radiant heat of the free molecule will maintain itself after that molecule has relinquished its freedom and formed part of a liquid. If, on the other hand, the state of aggregation be of paramount importance, we may expect to find, on the part of liquids, a deportment altogether different from that of their vapours. Which of these views corresponds with the truth of nature, we have now to inquire.

Melloni examined the diathermancy of various liquids, but he employed for this purpose the flame of an oil-lamp covered by a glass chimney. His liquids, moreover, were contained in glass cells; hence, the radiation was profoundly modified before it entered the liquid at all, glass being impervious to a considerable part of the emission. In the examination of the question now before us, it was my wish to interfere as little as possible with the primitive emission, and an apparatus was therefore devised in which layers of liquids, of various thicknesses, could be enclosed between two polished plates of rock-salt.

The apparatus consists of the following parts:—A B C (fig. 108) is a plate of brass, 3·4 inches long, 2·1 inches wide, and 0·3 of an inch thick. Into it, at its corners, are rigidly fixed four upright pillars, furnished at the top with screws, for the reception of the nuts *q r s t*. D E F is a second plate of brass, of the same size as the former, and pierced with holes at its four corners, so as to enable it to slip over the four pillars of the plate A B C. Both these plates are perforated by circular apertures, *m n* and *o p*, 1·35 inch in diameter. G H I is a third plate of brass, of the same area as D E F, and, like it, having its centre and its corners perforated. The plate G H I is intended to separate the two plates of rock-salt which are to form the walls of the cell, and its thickness determines that of the

liquid layer The separating plate G H I was ground with the utmost accuracy, and the surfaces of the plates of salt were polished with extreme care, with a view to rendering the contact between the salt and the brass water-tight. In practice, however, it was found necessary to introduce washers of thin letter-paper between the plates of salt and the separating plate.

FIG. 108.



In arranging the cell for experiment the nuts *q r s t* are unscrewed, and a washer of india-rubber is first placed on A B C. On this washer is placed one of the plates of rock-salt. On the plate of rock-salt is laid the washer of letter-paper, and on this again the separating plate G H I. A second washer of paper is placed on this plate, then comes the second plate of salt, on which another india-rubber washer is laid. The plate D E F is finally slipped

over the columns, and the whole arrangement is tightly screwed together by the nuts *q r s t*.

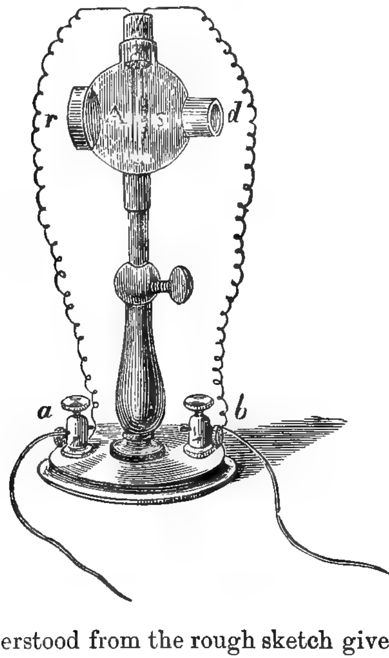
Thus, when the plates of rock-salt are in position, a circular space, as wide as the plate G H I is thick, is enclosed between them, and the space can be filled with any liquid through the orifice *k*. The use of the india-rubber washers is to relieve the crushing pressure which would be applied to the plates of salt, if they were in actual contact with the brass; and the use of the paper washers is, as already explained, to render the cell liquid-tight. After each experiment, the apparatus is unscrewed, the plates of salt are removed and thoroughly cleansed; the cell is then remounted, and in two or three minutes all is ready for a new experiment.

My next necessity was a perfectly steady source of heat, of sufficient intensity to penetrate the most absorbent of the liquids to be subjected to examination. This was found in a spiral of platinum wire, rendered incandescent by an electric current. The frequent use of this source led to the construction of the lamp shown in fig. 109. A is a globe of glass three inches in diameter, fixed upon a stand, which can be raised and lowered. At the top of the globe is an opening, into which a cork is fitted, and through the cork pass two wires, the ends of which are united by the platinum spiral *s*. The wires are carried down to the binding screws *a b*, which are fixed in the foot of the stand, so that when the instrument is attached to the battery, no strain is ever exerted on the wires which carry the spiral. The ends of the thick wire to which the spiral is attached are also of stout platinum, for when the ends were of copper, unsteadiness was introduced through oxidation. The heat issues from the incandescent spiral by the opening *d*, which is an inch and a half in diameter. Behind the spiral, finally, is a metallic reflector, *r*, which augments the flux of heat without sen-

sibly changing its quality. In the open air the red-hot spiral is a capricious source of heat, but surrounded by its glass globe its steadiness is admirable.[1]

The whole experimental arrangement will be imme-

FIG. 109.



diately understood from the rough sketch given in fig. 110. A is the platinum lamp just described, heated by a current from a Grove's battery of five cells. Means were devised to render this lamp perfectly constant throughout the day. In front of the spiral, and with an interior reflecting surface, is the tube B, through which the heat passes to the rock-salt cell C. This cell is placed on a little stage,

[1] I have also had lamps constructed in which the spirals were placed in vacuo, the rays passing to external space through a plate of rock-salt. Their steadiness is perfect.

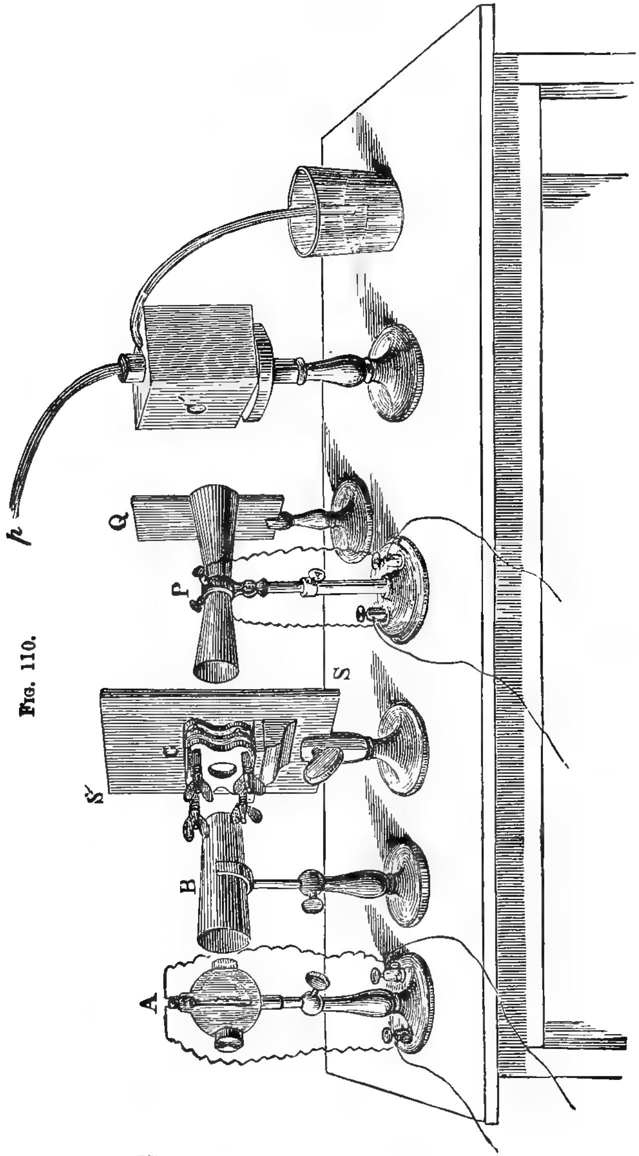


Fig. 110.

soldered to the back of the perforated screen s s′, so that the heat, after having crossed the cell, passes through the hole in the screen, and afterwards impinges on the thermo-pile P. C′ is the compensating cube, containing water kept boiling by steam passing through the pipe *p*. Between the cube C′ and the thermo-pile is the screen Q, which regulates the amount of heat falling on the posterior face of the pile. The whole arrangement is here exposed, but in practice, the pile P and the cube C′ are carefully protected from the capricious action of the surrounding air.

The experiments are thus performed. The empty rock-salt cell C being placed on its stage, a double silvered screen (not shown in the figure) is first introduced between the end of the tube B and the cell C; the heat of the spiral being thus totally cut off, and the pile subjected to the action of the cube C′ alone. By means of the screen Q the total heat to be adopted throughout the series of experiments is caused to fall on the posterior surface of the pile. Say that it corresponds to a galvanometric deflection of 50 degrees. The double screen used to intercept the radiation from the spiral is then gradually withdrawn, until this radiation completely neutralises that from the cube C′, and the needle of the galvanometer points steadily to zero. The position of the two screens, once fixed in this way, remains subsequently unchanged.

The rays in the first instance pass from the spiral through the *empty* rock-salt cell. The drawn-out shank of a small funnel, suitably supported, dips into the aperture *k*, fig. 108, which leads into the cell, and through this funnel the liquid is poured in. The introduction of the liquid destroys the previous equilibrium, the galvanometer needle moves, and finally assumes a steady position. From its deflection we can immediately calculate the quantity of heat absorbed by the liquid, and express it in hundredths of the entire radiation.

The experiments were executed with eleven different liquids, employing, with a view to verification, each liquid in five different thicknesses. The results are collected together in the following table:—

ABSORPTION OF HEAT BY LIQUIDS. SOURCE OF HEAT: PLATINUM SPIRAL RAISED TO BRIGHT REDNESS BY A VOLTAIC CURRENT.

Liquid	Thickness of liquid in parts of an inch				
	0·02	0·04	0·07	0·14	0·27
Bisulphide of carbon	5·5	8·4	12·5	15·2	17·3
Chloroform	16·6	25·0	35·0	40·0	44·8
Iodide of methyl	36·1	46·5	53·2	65·2	68·6
Iodide of ethyl	38·2	50·7	59·0	69·0	71·5
Benzol	43·4	55·7	62·5	71 5	73·6
Amylene	58·3	65·2	73·6	77·7	82·3
Sulphuric ether	63·3	73·5	76·1	78·6	85·2
Acetic ether	—	74·0	78·0	82·0	86·1
Formic ether	65·2	76·3	79·0	84·0	87·0
Alcohol	67·3	78·6	83·6	85·3	89·1
Water	80·7	86·1	88·8	91·0	91·0

Were it necessary to push these experiments to the utmost limits of accuracy, I should make each measurement several times and take the mean of the determinations. But, considering the way in which the different thicknesses check each other, the results obviously express, with close approximation to the truth, the action of the respective liquids. The *order* of absorption is certainly that here established.

As liquids, then, those bodies are shown to intercept in very different degrees the heat emitted by our radiating source; and we have next to inquire whether these differences continue, after the molecules have been released from the bond of cohesion and reduced to the state of vapour. We must, of course, test the vapours by waves of the same period as those applied to the liquids, and this our mode of experiment renders easy of accomplishment.

The heat generated in a wire by a current of a given strength being invariable, it was only necessary, by means of a tangent compass and rheocord, to keep the current constant from day to day, in order to obtain, both as regards quantity and quality, an invariable source of heat.

The liquids from which the vapours were derived were placed in the small flasks already described, a separate flask being devoted to each. After the complete removal of the air, the flasks were attached in succession to the brass experimental tube. With the single exception that the source of heat was a red-hot platinum spiral, instead of a cube of hot water, the arrangement was that figured in the frontispiece. At the commencement of each experiment, the brass tube being thoroughly exhausted, and the radiation from the spiral being neutralised by the compensating cube, the needle stood at zero. The cock of the flask containing the volatile liquid was then carefully turned on, and the vapour allowed slowly to enter the experimental tube, until a pressure of 0·5 of an inch was obtained. The vapour was then cut off, and the permanent deflection noted. Knowing the total heat, the absorption in 100ths of the entire radiation could be at once deduced from the deflection. The following table contains the results:—

RADIATION OF HEAT THROUGH VAPOURS. SOURCE: RED-HOT PLATINUM SPIRAL. PRESSURE, 0·5 OF AN INCH.

	Absorption per cent.
Bisulphide of carbon	4·7
Chloroform	6·5
Iodide of methyl	9·6
Iodide of ethyl	17·7
Benzol	20·6
Amylene	27·5
Alcohol	28·1
Formic ether	31·4
Sulphuric ether	31·9
Acetic ether	34·6
Total heat	**100·0**

We are now in a condition to compare the action of a series of volatile liquids, with that of the vapours of those liquids, upon radiant heat. Beginning with the substance of the lowest absorptive energy, and proceeding to the highest, we have the following orders of absorption:—

Liquids	Vapours
Bisulphide of carbon.	Bisulphide of carbon.
Chloroform.	Chloroform.
Iodide of methyl.	Iodide of methyl.
Iodide of ethyl.	Iodide of ethyl.
Benzol.	Benzol.
Amylene.	Amylene.
Sulphuric ether.	Alcohol.
Acetic ether.	Formic ether.
Formic ether.	Sulphuric ether.
Alcohol.	Acetic ether.
Water.	

Here, as far as amylene, the order of absorption is the same for both liquids and vapours. But from amylene downwards, though strong liquid absorption is, in a general way, paralleled by strong vapour absorption, the order of both is not the same. There is not the slightest doubt that, next to water, alcohol is the most powerful absorber in the list of liquids; but there is just as little doubt that the position which it here occupies in the list of vapours is the correct one. This has been established by reiterated experiments. Acetic ether, on the other hand, though certainly the most energetic absorber in the state of vapour, falls behind both formic ether and alcohol in the liquid state. Still, on the whole, it is perfectly impossible to contemplate these results, without arriving at the conclusion that the act of absorption is, in the main, *molecular*, and that the molecules maintain their power as absorbers and radiators when they change their state of aggregation. Should any doubt, however, linger as to

the correctness of this conclusion, it will speedily disappear.

A moment's reflection will show that the comparison here instituted is not a strict one. We have taken the liquids at a common thickness, and the vapours at a common volume and pressure. But if the layers of liquids employed were turned into vapour, the volumes obtained would *not* be the same. The quantities of matter traversed by the radiant heat are not proportional to each other in the two cases, and to render the comparison strict they ought to be proportional. It is easy, of course, to make them so; for the liquids being examined at a constant volume, their specific gravities give us the relative quantities of matter traversed by the radiant heat, and from these and the vapour-densities, we can immediately deduce the corresponding volumes of the vapour. Dividing, in fact, the specific gravities of our liquids by the densities of their vapours, we obtain the following series of vapour volumes, whose weights are proportional to the masses of liquid employed.

TABLE OF PROPORTIONAL VOLUMES.

Bisulphide of carbon	0·48
Chloroform	0·36
Iodide of methyl	0·46
Iodide of ethyl	0·36
Benzol	0·32
Amylene	0·26
Alcohol	0·50
Sulphuric ether	0·28
Formic ether	0·36
Acetic ether	0·29
Water	1·60

Introducing the vapours, in the volumes here indicated, into the experimental tube, the following results were obtained:—

RADIATION OF HEAT THROUGH VAPOURS. QUANTITY OF VAPOUR PROPORTIONAL TO THAT OF LIQUID.

Name of Vapour	Pressure in parts of an inch	Absorption per cent.
Bisulphide of carbon	0·48	4·3
Chloroform	0·36	6·6
Iodide of methyl	0·46	10·2
Iodide of ethyl	0·36	15·4
Benzol	0·32	16·8
Amylene	0·26	19·0
Sulphuric ether	0·28	21·5
Acetic ether	0·29	22·2
Formic ether	0·36	22·5
Alcohol	0·50	22·7

Arranging both liquids and vapours in the order of their absorption, we now obtain the following result :—

Liquids	Vapours
Bisulphide of carbon.	Bisulphide of carbon.
Chloroform.	Chloroform.
Iodide of methyl.	Iodide of methyl.
Iodide of ethyl.	Iodide of ethyl.
Benzol.	Benzol.
Amylene.	Amylene.
Sulphuric ether.	Sulphuric ether.
Acetic ether.	Acetic ether.
Formic ether.	Formic ether.
Alcohol.	Alcohol.
Water.	[1]

Here the discrepancies revealed by our former series of experiments entirely disappear, and it is proved that for heat of the same quality, the order of absorption for liquids and their vapours is the same. We may, therefore, safely infer that the position of a vapour, as an absorber or a radiator, is determined by that of the liquid from which it is derived. Granting the validity of this inference, the position of *water* fixes that of *aqueous vapour*. But we have found that, for all thicknesses, water exceeds every other liquid in the energy of its absorption. Hence,

[1] Aqueous vapour, unmixed with air, condenses so readily that it cannot be directly examined in our experimental tube.

if no single experiment on the vapour of water existed, we should conclude, from the deportment of its liquid, that, weight for weight, aqueous vapour transcends all others in absorptive power. Add to this the direct and multiplied experiments, by which the action of this substance on radiant heat has been established, and we have before us a body of evidence sufficient, I trust, to set this question for ever at rest, and to induce the meteorologist to apply the result, without misgiving, to the phenomena of his science.

We must now prepare the way for the consideration of an important question. A pendulum swings at a certain definite rate, depending on the length of the pendulum. A spring oscillates at a rate which depends upon its weight and elastic force. A musical string, in like manner, has its determining rate of vibration, which depends upon its length, weight and tension. A plank which bridges a gorge has also its own rate of oscillation, and we can often, by timing our movements on such a plank, so accumulate the impulses as to endanger its safety. Soldiers, in crossing pontoon bridges, tread irregularly, lest the motion imparted to the pontoon, should accumulate to a dangerous extent. The step of a person carrying water on his head in an open pail sometimes coincides with the oscillation of the water from side to side of the vessel, until, impulse being added to impulse, the liquid finally splashes over the rim. The water carrier instinctively alters his step, and thus reduces the liquid to comparative tranquillity. You have heard a particular pane of glass respond to a particular note of an organ: and if you open a piano and sing into it, some one string will also respond. In the case of the organ, the pane responds because its period of vibration happens to coincide with the period of the sonorous waves that impinge upon it; and in the case of the piano, that

string responds whose period of vibration coincides with the period of the vocal chords of the singer. In each case, there is an accumulation of the effect, similar to that observed when you stand upon a plank-bridge, and time your impulses to its rate of vibration. In the case of the singing flame referred to in a former lecture, you had the influence of period exemplified in a very striking manner. It responded to the voice only when the pitch of the voice corresponded to its own. A higher and a lower note were equally ineffective to put the flame in motion.

These ordinary mechanical and acoustical facts will help us to an insight as to the more subtle phenomena of light and radiant heat. I have shown you the diathermancy of lampblack and bromine, and the far more wonderful transparency of iodine shall be amply illustrated by-and-by. We have now to inquire why iodine stops light and allows heat to pass. The sole difference between light and radiant heat is one of period. The waves of the one are short and of rapid recurrence, while those of the other are long and of slow recurrence. The former are intercepted by the iodine, and the latter transmitted. Why? There can, I think, be only one answer to this question, namely, that the intercepted waves are those whose periods coincide with the periods of oscillation possible to the atoms of the iodine. The waves transfer their motion to the atoms which synchronise with them. Supposing waves of any period to impinge upon an assemblage of molecules of any other period, it is, I think, physically certain that a *tremor* of greater or less intensity will be set up among the molecules; but for the motion to accumulate, so as to produce sensible absorption, coincidence of period is necessary. Briefly defined, therefore, transparency is synonymous with *discord*, while opacity is synonymous with *accord* between the periods of the waves of ether and those of the atoms of the body on which they impinge.

The term 'quality,' as applied to radiant heat, has been already defined. If two calorific beams be transmitted by the same substance in different proportions, the beams are said to be of different qualities. Strictly speaking, this question of quality is one of period; and if the heat of one source be more or less copiously transmitted than the heat of another source, it is because the waves of ether excited by the one are different in length and period from those excited by the other. When we raise the temperature of our platinum spiral, we alter the quality of its heat. As the temperature is raised, shorter and ever shorter waves mingle in the radiation. Dr. Draper, in a very beautiful investigation, has shown that when platinum first appears luminous, it emits only red rays; but as its temperature augments, orange, yellow, and green are successively added to the radiation; and when the platinum is so intensely heated as to emit white light, the decomposition of that light gives all the colours of the solar spectrum.

Almost all the vapours which we have hitherto examined are transparent to light, while all of them are, in some degree, opaque to obscure heat-rays. From this the incompetence of the vapour molecules to vibrate in visual periods, and their competence to vibrate in slower periods may be inferred. Conceive, then, our platinum spiral to be gradually raised from a state of obscure to a state of luminous heat; the change would manifestly tend to produce *discord* between the radiating platinum and the molecules of our vapours. On *à priori* grounds, then, we should infer, that the raising of the temperature of the platinum spiral ought to augment the power of its rays to pass through our list of vapours. In other words, the transparency of the vapours ought to increase with the temperature. This conclusion is entirely verified by the experiments recorded in the following tables:—

RADIATION THROUGH VAPOURS. SOURCE OF HEAT: PLATINUM SPIRAL BARELY VISIBLE IN THE DARK.

Name of Vapour	Absorption per cent.
Bisulphide of carbon	6·5
Chloroform	9·1
Iodide of methyl	12·5
Iodide of ethyl	21·0
Benzol	26·3
Amylene	35·8
Sulphuric ether	43·4
Formic ether	45·2
Acetic ether	49·6

With the same platinum spiral raised to a white heat, the following results were obtained:—

RADIATION THROUGH VAPOURS. SOURCE OF HEAT: WHITE-HOT PLATINUM SPIRAL.

Name of Vapour	Absorption per cent.
Bisulphide of carbon	2·9
Chloroform	5·6
Iodide of methyl	7·8
Iodide of ethyl	12·8
Benzol	16·5
Amylene	22·7
Formic ether	25·1
Sulphuric ether	25·9
Acetic ether	27·2

With the same spiral, brought still nearer to its point of fusion, the following results were obtained with four of the vapours:—

RADIATION THROUGH VAPOURS. SOURCE: PLATINUM SPIRAL AT AN INTENSE WHITE HEAT.

Name of Vapour	Absorption
Bisulphide of carbon	2·5
Chloroform	3·9
Formic ether	21·3
Sulphuric ether	23·7

Placing the results obtained with the respective sources

side by side, the influence of the vibrating period on the transmission comes out in a very decided manner :—

ABSORPTION OF HEAT BY VAPOURS.

Name of Vapour	Source : Platinum Spiral			
	Barely visible	Bright red	White-hot	Near fusion
Bisulphide of carbon	6·5	4·7	2·9	2·5
Chloroform . . .	9·1	6·3	5·6	3·9
Iodide of methyl . .	12·5	9·6	7·8	
Iodide of ethyl . .	21·0	17·7	12·8	
Benzol	26·3	20·6	16·5	
Amylene . .	35·8	27·5	22·7	
Sulphuric ether . .	43·4	31·4	25·9	23·7
Formic ether . . .	45·2	31·9	25·1	21·3
Acetic ether . . .	49·6	34·6	27·2	

The gradual augmentation of penetrative power, as the temperature is augmented, is here very manifest. By raising the spiral from a barely visible heat to an intense white heat, we reduce the absorption, in the case of bisulphide of carbon and chloroform, to less than one-half. At barely visible redness, moreover, 56·6 and 54·8 per cent. pass through sulphuric and formic ether respectively; while of the intensely white-hot spiral, 76·3 and 78·7 per cent. pass through the same vapours.[1] Thus, by augmenting the temperature of the platinum, we introduce into the radiation waves of shorter period, which, being in discord with the periods of the vapour molecules, pass more easily among them.

Running the eye along the numbers which express the absorptions of sulphuric and formic ether in the last table, we find that, for the lowest heat, the absorption of the latter exceeds that of the former; for a bright red heat they are nearly equal, the formic ether still retaining a slight predominance; at a white heat, however, the sulphuric slips in advance, and at the heat of fusion its predominance is decided. I have tested this result by

[1] The *transmission* is found by subtracting the absorption from 100.

multiplied experiments, and placed it beyond doubt. We may at once infer from it that the capacity of the molecule of formic ether to enter into rapid vibration is less than that of sulphuric ether. The more we heighten the temperature of the spiral, the more opaque, in comparison with formic ether, does sulphuric ether become. Experiments made with a source of 100° C., establish still more decidedly the preponderance of the formic ether for vibrations of slow period. This is shown in the following table :—

RADIATION THROUGH VAPOURS. SOURCE: LESLIE'S CUBE, COATED WITH LAMPBLACK. TEMPERATURE, 100° C.

Name of Vapour	Absorption per cent.
Bisulphide of carbon	6·6
Iodide of methyl	18·8
Chloroform	21·6
Iodide of ethyl	29·0
Benzol	34·5
Amylene	47·1
Sulphuric ether	54·1
Formic ether	60·4
Acetic ether	69·9

For heat issuing from this source, the absorption by formic ether is 6·3 per cent. in excess of that by sulphuric.

But in this table we notice another case of reversal. In all the experiments with the platinum spiral, chloroform showed itself less energetic than iodide of methyl; but with lampblack as a source, chloroform shows itself to be decidedly the more powerful absorber of the two.

RADIATION FROM FLAMES.

We have hitherto occupied ourselves with the radiation from heated solids: let us now pass on to the examination of the radiation from flames. The earliest of these experiments were made with a steady jet of gas, issuing from a small circular burner, the flame being long and tapering. The

top and bottom of the flame were excluded, and its most brilliant portions were chosen as the source. The results obtained are recorded in the following table :—

RADIATION OF HEAT THROUGH VAPOURS. SOURCE: A HIGHLY LUMINOUS JET OF GAS.

Name of Vapour	Absorption	White-hot Spiral
Bisulphide of carbon . .	9·8	2·9
Chloroform	12·0	5·6
Iodide of methyl . . .	16·5	7·8
Iodide of ethyl . . .	19·5	12·8
Benzol.	22·0	16·5
Amylene	30·2	22·7
Formic ether . . .	34·6	25·9
Sulphuric ether . . .	35·7	25·1
Acetic ether . . .	38·7	27·2

To facilitate the comparison of the white-hot carbon with the white-hot platinum, the results obtained with both are placed side by side. The emission from the flame is here proved to be far more powerfully absorbed than the emission from the spiral. Doubtless, however, the carbon, in reaching incandescence, passes through lower stages of temperature, and in those stages emits heat more in accord with the vapours. It is also mixed with the vapour of water and carbonic acid, both of which contribute their quota to the total radiation. It is therefore probable that the greater absorption of the heat emitted by the flame is in part due to the slower periods of the molecules, which are unavoidably mixed with the white-hot carbon.

The next source of heat employed was the flame of a Bunsen's burner,[1] the temperature of which is known to be very high. The flame was of a pale blue colour, and emitted a very feeble light. The following results were obtained :—

[1] Described in Lecture III.

RADIATION OF HEAT THROUGH VAPOURS. SOURCE: PALE-BLUE FLAME OF BUNSEN'S BURNER.

Name of Vapour	Absorption
Chloroform	6·2
Bisulphide of carbon	11·1
Iodide of ethyl	14·0
Benzol	17·9
Amylene	24·2
Sulphuric ether	31·9
Formic ether	33·3
Acetic ether	36·3

Comparing the last two tables, we see that the radiation from Bunsen's flame is, on the whole, less powerfully absorbed than that from the luminous gas jet. In some cases, as in that of formic ether, they come very close to each other; in the case of amylene, and a few other substances, they differ more markedly. But an extremely interesting case of reversal here shows itself. Bisulphide of carbon, instead of being first, stands decidedly below chloroform. With the luminous jet, the absorption of bisulphide of carbon is to that of chloroform as 100 : 122, while with the flame of Bunsen's burner the ratio is 100 : 56. The removal of the carbon from the flame more than doubles the relative transparency of the chloroform. We have here, moreover, another instance of the reversal of formic and sulphuric ether. For the luminous jet, the sulphuric ether is decidedly the more opaque; for the flame of Bunsen's burner, it is excelled in opacity by the formic.

The main radiating bodies in the flame of a Bunsen's burner are, no doubt, aqueous vapour and carbonic acid. I wished to separate these two constituents, and to study them separately. The radiation of aqueous vapour could be obtained from a flame of pure hydrogen, while that of carbonic acid could be obtained from an ignited jet of carbonic oxide. Notwithstanding the high temperature of

the hydrogen flame, I thought it likely that the accord between its periods of vibration and those of the cool aqueous vapour of the atmosphere would still be such as to cause the atmospheric vapour to exert a special absorbent power upon the radiation. The following experiments establish the truth of this surmise.

RADIATION THROUGH ATMOSPHERIC AIR. SOURCE: A HYDROGEN FLAME.

	Absorption
Dry air	0
Undried air	17·2

Thus, in a polished tube 4 feet long, the aqueous vapour of our laboratory air absorbed 17 per cent. of the radiation from the hydrogen flame. When a platinum spiral, raised by electricity to a degree of incandescence not greater than that attainable by plunging a wire into the hydrogen flame, was used as a source of heat, the undried air of the laboratory was found to absorb

5·8 per cent.

of its radiation, or one-third of the quantity absorbed in the case of the flame of hydrogen.

The plunging of a spiral of platinum wire into the flame reduces its temperature; but the spiral at the same time sends forth vibrations, which are not in accord with those of aqueous vapour. The absorption, by ordinary undried air, of heat emitted by this composite source amounted to

8·6 per cent.

On humid days, the absorption of the heat emitted by a hydrogen flame exceeds even the largest figure above recorded. Employing the same experimental tube and a new burner, the experiments were repeated some days subsequently, with the following result:—

RADIATION THROUGH AIR. SOURCE: HYDROGEN FLAME.

	Absorption
Dry air	0
Undried air	20·3

We may infer from the foregoing powerful action of atmospheric vapour on the radiation from the hydrogen flame, that synchronism reigns between the molecular vibrations of the flame at a temperature (according to Bunsen) of 5898° Fahr. and those of aqueous vapour at a temperature of 60° Fahr. The enormous temperature of the hydrogen flame increases the atomic amplitude or width of swing, but does not change the period of oscillation.

The other component of the flame of Bunsen's burner is carbonic acid, and the radiation of this substance is immediately obtained from a flame of carbonic oxide. Of the radiation from this source, the small amount of carbonic acid diffused in the air of our laboratory absorbed 13·8 per cent. This high absorption proves that the vibrations of the molecules of carbonic acid, within the flame, are synchronous with the vibrations of those of the carbonic acid of the atmosphere. Bunsen makes the temperature of the flame 5508° Fahr., while that of the atmosphere is only 60°. But if the high temperature is incompetent to change the rate of oscillation, we may expect cold carbonic acid, when used in large quantities, to be highly opaque to the radiation from the carbonic oxide flame. Here follow the results of experiments executed to test this conclusion :—

RADIATION THROUGH DRY CARBONIC ACID. SOURCE: CARBONIC OXIDE FLAME.

Pressure in inches	Absorption
1·0	48·0
2·0	55·5
3·0	60·3
4·0	65·1
5·0	68 6
10·0	74·3

For the rays emanating from the heated solids employed in our former researches, carbonic acid proved to be one of the most feeble absorbers; but here, when the waves sent into it emanate from molecules of its own substance, its absorbent energy is enormous. The thirtieth of an atmosphere of the gas cuts off half the entire radiation; while at a pressure of 4 inches, 65 per cent. of the radition is intercepted.

The energy of olefiant gas, both as an absorbent and as a radiant, is now well known to you. For the solid sources of heat just referred to, its power is far greater than that of carbonic acid; but for the radiation from the carbonic oxide flame, the power of olefiant gas is feeble, when compared with that of carbonic acid. This is proved by the experiments recorded in the following table:—

RADIATION THROUGH DRY OLEFIANT GAS AND DRY CARBONIC ACID.
SOURCE: CARBONIC OXIDE FLAME.

Pressure in inches	Olefiant gas absorption	Carbonic acid absorption
1·0	23·2	48·0
2·0	34·7	55·5
3·0	44·0	60·3
4·0	50·6	65·1
5·0	55·1	68·6
10·0	65·5	74·3

Olefiant gas and carbonic acid are placed here side by side. The superior power of the acid is very decided, and most so at the smaller pressures. At a pressure of an inch it is twice that of the olefiant gas. The substances agree more closely, as the quantity of gas augments. Here, in fact, both of them approach perfect opacity, and as they draw near to this common limit, their absorptions, as a matter of course, approximate.

The presence of an infinitesimal quantity of carbonic acid gas may be detected, by its action on the rays emitted by a carbonic oxide flame. The action, for ex-

ample, of the carbonic acid expired by the lungs is very decided. An india-rubber bag was filled by the breath; it contained, therefore, both the aqueous vapour and the carbonic acid of the breath. The air from the bag was then conducted through a drying apparatus, the moisture being thus removed, and the neutral air and active carbonic acid permitted to enter the experimental tube. The following results were obtained :—

Human Breath containing CO^2. Source: Carbonic Oxide Flame.

Pressure in inches	Absorption
1	12·0
2	25·0
5	33·3
30	50·0

Thus, the tube filled with the dried breath intercepted 50 per cent. of the entire radiation from a carbonic oxide flame. It is quite manifest that we have here a means of testing, with surpassing delicacy, the amount of carbonic acid emitted under various circumstances from the lungs.

This mode of experiment was further illustrated by Mr. Barrett, when he was my assistant. The deflection produced by the breath, freed from its moisture, but retaining its carbonic acid, was first determined. Carbonic acid, artificially prepared, was then mixed with perfectly dry air, in such proportions that its action upon the radiant heat was the same as that of the carbonic acid of the breath. The proportions of the artificial mixture gave those of the breath. Here follow the results of three chemical analyses, determined by Dr. Frankland, as compared with three physical analyses performed by my late assistant.

Percentage of Carbonic Acid in Human Breath.

By chemical analysis	By physical analysis
4·311	4·00
4·66	4·56
5·33	5·22

The agreement between the results is very fair. Doubtless, with greater practice a closer agreement could be attained.

Water at moderate thickness is a very transparent substance; that is to say, its vibrating periods are, as already explained, in discord with those of the visible spectrum It is also highly transparent to the ultra-violet rays. For the rays of the invisible spectrum beyond the red, the opacity of the substance is, on the other hand, unequalled. The synchronism of the vibrating periods of the water molecules with those of the ultra-red waves is thus demonstrated. But from the deportment of undried atmospheric air we have already inferred the synchronism of the aqueous vapour of the air, and the hot vapour of the flame. If, therefore, the periods of a vapour be the same as those of its liquid, we ought to find water also highly opaque to the radiation from a hydrogen flame. Here are the results obtained with five different thicknesses of the liquid:—

RADIATION THROUGH WATER. SOURCE: HYDROGEN FLAME.

	Thickness of liquid				
	0·02 inch	0·04 inch	0·07 inch	0·14 inch	0·27 inch
Transmission per cent.	5·8	2·8	1·1	0·5	0·0

Through a layer of water 0·36 of an inch thick, Melloni found a transmission of 11 per cent. for the heat of an Argand lamp. Here we employ a source of higher temperature, and a layer of water only 0·27 of an inch, and find the whole of the heat intercepted. A layer of water, moreover, about one-tenth of the thickness of that employed by Melloni, permits of a transmission of only 3 per cent. of the entire radiation. Hence we may infer the coincidence in vibrating period between cold water and aqueous vapour heated to a temperature of 5898° Fahr. (3259° C.)

From the opacity of water to the radiation from aqueous vapour, we may infer the opacity of aqueous vapour to the radiation from water, and hence conclude that the very act of nocturnal refrigeration which causes the condensation of water on the earth's surface, gives to terrestrial radiation that particular character which renders it most liable to be intercepted by our atmosphere, and thus prevented from wasting itself in space.

INFLUENCE OF AN ATMOSPHERE ON PLANETARY TEMPERATURE

This is a point which deserves a moment's further consideration. I find that a layer of olefiant gas 2 inches thick absorbs 33 per cent., a layer 1 inch thick absorbs 26 per cent., while a layer $\frac{1}{100}$th of an inch in thickness absorbs 2 per cent. of the radiation from an obscure source. Let us now consider for a moment the effect upon the earth's temperature of a shell of olefiant gas, surrounding our planet at a little distance above its surface. The gas would be transparent to the solar rays, allowing them, without sensible hindrance, to reach the earth. Here, however, the luminous heat of the sun would be converted into non-luminous terrestrial heat; at least 26 per cent. of this heat would be intercepted by a layer of gas one inch thick, and in great part returned to the earth. Under such a canopy, trifling as it may appear, and perfectly transparent to the eye, the earth's surface would be maintained at a stifling temperature.

A few years ago, a work possessing great charms of style and ingenuity of reasoning, was written to prove that the more distant planets of our system are uninhabitable. Applying the law of inverse squares to their distances from the sun, the diminution of temperature was found to be so great, as to preclude the possibility of human life in the more remote members of the solar

system. But in those calculations the influence of an atmospheric envelope was overlooked, and this omission vitiated the entire argument. An atmosphere may act the part of a *barb* to the solar rays, permitting them to reach the earth, but preventing their escape. A layer of air two inches in thickness, saturated with the vapour of sulphuric ether, would offer very little resistance to the passage of the solar rays, but I find that it would cut off fully 35 per cent. of the planetary radiation. It would require no inordinate thickening of the layer of vapour to double this absorption; and it is perfectly evident that, with a protecting envelope of this kind, permitting the heat to enter, but preventing its escape, a comfortable temperature might be obtained on the surface of the most distant planet.

The late Dr. Miller was the first to infer from the inability of the rays of burning hydrogen to pass through glass screens, that the vibrating periods of the flame must be in the main ultra-red; and that consequently, the oscillating periods of the lime-light must be more rapid than those of the oxyhydrogen flame to which it owes its incandescence.[1] As pointed out by Dr. Miller, the incandescent lime furnishes an example of exalted refrangibility. The same remark applies to a platinum wire

[1] After referring to the researches of Professor Stokes on 'degraded' refrangibility, Dr. Miller says:—'Heat of low refrangibility may, however, be converted into heat of higher refrangibility: for example, a jet of mixed oxygen and hydrogen gases furnishes a heat nearly as intense as any which art can command, yet it does not emit rays which have the power of traversing glass in any considerable quantity, even though a lens be employed for their concentration. Upon introducing a cylinder of lime into the jet of burning gases, though the amount of heat is not thus increased, the light becomes too bright for the unprotected eye to endure, and the thermic rays acquire the property of traversing glass, as is shown by their action upon a thermometer the bulb of which is placed in the focus of the lens.'—*Chemical Physics*, 1855, p. 210.

plunged in a hydrogen flame. We have, in this case also, a conversion of unvisual periods into visual ones. This shortening of the periods must augment the discord between the radiating source and the series of liquids with which we have operated, and hence augment their transparency to the radiation. The conclusion was tested and verified by experiments on layers of the liquids of two different thicknesses.

RADIATION THROUGH LIQUIDS. SOURCES: 1. HYDROGEN FLAME; 2. HYDROGEN FLAME AND PLATINUM SPIRAL.

Name of liquid	Transmission			
	Thickness of liquid 0·04 inch: Flame only.	Flame and spiral.	Thickness of liquid 0·07 inch. Flame only.	Flame and spiral.
Bisulphide of carbon	77·7	87·2	70·4	86·0
Chloroform . .	54·0	72·8	50·7	69·0
Iodide of methyl .	31·6	42·4	26·2	36·2
Iodide of ethyl .	30·3	36·8	24·2	32·6
Benzol . . .	24·1	32·6	17·9	28·8
Amylene . . .	14·9	25·8	12·4	24·3
Sulphuric ether .	13·1	22·6	8·1	22·0
Acetic ether . .	10·1	18·3	6·6	18·5
Alcohol . . .	9·4	14·7	5·8	12·3
Water . . .	3·2	7·5	2·0	6·4

The transmission in each case is shown to be considerably augmented by the introduction of the platinum wire.

SPECTRUM OF HYDROGEN FLAME.

Direct experiments with the hydrogen-flame completely verify the inference of Dr. Miller. I had constructed a complete rock-salt train of lenses and prisms, capable of being substituted for the ordinary glass train of the electric lamp. Within the camera of the lamp was placed a burner with a single circular aperture, the flame issuing from it occupying the position usually taken up by the carbon-points. This burner was connected with a T-piece, from which two pieces of india-rubber tubing were carried, the one to a large hydrogen-holder, the other to

the gas-pipe of the laboratory. It was thus in my power to have, at will, either the gas-flame or the hydrogen-flame. When the former was employed, a visible spectrum was produced, which enabled me to fix the thermo-pile in its proper position. To obtain the latter, it was only necessary to turn on the hydrogen until it reached the gas-flame; then to turn off the gas and leave the hydrogen-flame behind. In this way the one flame could be substituted for the other without opening the door of the camera, or producing any change in the position of the source, the lenses, the prism, or the pile.

The spectrum of the luminous gas-flame being cast upon the screen, the linear thermo-pile was gradually moved through the colours and beyond the red, until the deflection of the galvanometer became a maximum. The deflection then observed was

30°

When the pile was moved in either direction from this position, the deflection diminished.

The hydrogen-flame was now substituted for the gas-flame; the visible spectrum disappeared, and the deflection fell to

12°.

Hence, as regards rays of this particular refrangibility, the emission from the luminous gas-flame was two-and-a-half times that from the hydrogen-flame.

The pile was again moved to and fro, the movement in both directions being accompanied by a diminished deflection. Twelve degrees, therefore, was the maximum deflection for the hydrogen-flame; and the position of the pile, determined previously by means of the luminous flame, proves that this deflection was produced by ultra-red heat. I moved the pile a little forwards, so as to reduce the deflection from 12° to 4°, and then, in order to ascertain the refrangi-

bility of the rays which produced this small deflection, re-lighted the gas. The face of the pile was found invading the red. When the pile was caused to pass successively through positions corresponding to the various colours of the spectrum, and to its ultra-violet rays, no measurable deflection was produced by the hydrogen-flame.

It is thus conclusively proved that the radiation from a hydrogen-flame, as far as it is capable of measurement by our delicate arrangement, is ultra-red. The other constituents of the radiation are so feeble as to be thermally insensible.

And here we find ourselves in a position to offer solutions of various facts, which have hitherto stood out as enigmas in researches upon radiant heat. It was for a time generally supposed that the power of heat to penetrate diathermic substances augumented as the temperature of the source became more elevated. Knoblauch, whose beautiful researches on Radiant heat are so well known and appreciated, contended against this notion, showing that the heat emitted by a platinum wire plunged in an alcohol flame was less absorbed by certain diathermic substances, than the heat of the flame itself; and he justly argued that the temperature of the spiral could not be higher than that of the body from which its heat was derived. A plate of transparent glass being introduced between the flame with its incandescent platinum spiral and the thermo-pile, the deflection of his needle fell from 35° to 19°; while, when the source was the flame of alcohol, without the spiral, the deflection fell from 35° to 16°. This proved the radiation from the alcohol flame to be more intercepted than that from the spiral; or, in other words, that the heat emanating from the body of highest temperature possessed the least penetrative power. Melloni afterwards verified this experiment.

Transparent glass of the kind employed by Melloni allows the rays of the visible spectrum to pass freely through it; but it is highly opaque to the radiation from obscure sources. A plate 0·1 of an inch thick intercepts all the rays from a source of 100° C., and transmits only 6 per cent. of the heat emitted by a source of 400° C. (see p. 308). Now the products of an alcohol flame are aqueous vapour and carbonic acid, whose waves have been proved to be of the particular character most powerfully intercepted by glass. But by plunging a platinum wire into such a flame, we convert its heat into heat of higher refrangibility; and thus establish the discord between the periods of the source and the periods of the glass, which, as before defined, is the physical cause of transparency. On purely *à priori* grounds, therefore, we might infer that the introduction of the platinum spiral would augment the penetrative power of the heat. With a plate of glass Melloni, in fact, found the following transmissions for the flame and the red-hot spiral:—

For the flame	For the platinum
41·2	52·8

The same remarks apply to the transparent selenite examined by Melloni. This substance is highly opaque to the ultra-red undulations; but the radiation from an alcohol flame is mainly ultra-red, hence the opacity of the selenite to this radiation. The introduction of the platinum spiral quickens the periods of oscillation and augments the transmission. Melloni found the transmissions through selenite to be as follows:—

Flame	Platinum
4·4	19·5

So far the results of Melloni coincide with those of Knoblauch; but the Italian philosopher pursued the matter further, and showed that Knoblauch's results, though true for the particular substances examined by him, are

not true of diathermic media universally. In the case of *black* glass and *black* mica, a striking inversion of the effect was observed. Through these substances the radiation from the flame was more copiously transmitted than that from the platinum. For black Melloni found the following transmissions:—

From the flame	From the platinum
52·6	42·8

And for a plate of black mica the following transmissions:—

From the flame	From the platinum
62·8	52·5

These results were left unexplained by Melloni, but the solution is now easy. The black glass and the black mica owe their blackness to the carbon incorporated in them, and it has been shown that while opaque to light carbon is, in a considerable degree, pervious to the waves of long period; that is to say, to such waves as are emitted by a flame of alcohol. The case of the carbon is therefore precisely antithetical to that of the transparent glass, the former transmitting the heat of long period, and the latter that of short period most freely. Hence it follows that the introduction of the platinum spiral, by converting the long periods of the flame into short ones, augments the transmission through the transparent glass and selenite, and diminishes it through the opaque glass and mica.

LECTURE XV.

DISCOVERY OF DARK SOLAR RAYS—HERSCHEL'S AND MÜLLER'S EXPERIMENTS—RISE OF INTENSITY WITH TEMPERATURE—HEAT OF ELECTRIC SPECTRUM—RAY-FILTERS: SIFTING THE ELECTRIC LIGHT—TRANSMUTATION OF RAYS—THERMAL IMAGE RENDERED LUMINOUS—COMBUSTION AND INCANDESCENCE BY DARK RAYS—FLUORESCENCE AND CALORESCENCE—DARK SOLAR RAYS—DARK LIME-LIGHT RAYS—FRANKLIN'S EXPERIMENT ON COLOURS—ITS ANALYSIS AND EXPLANATION.

INVISIBLE SOLAR RADIATION.

ON a former occasion I promised to make known to you the progress of recent inquiry as regards the subject of invisible radiation. A hope was then expressed that I should be able to sift in your presence the composite emission of the electric lamp; to detach its rays of darkness from its rays of light, and to show you the power of those dark rays when they are properly intensified and concentrated.

The hour now before us shall be devoted to an attempt to redeem this promise. And in the first place it is necessary that we should have distinct notions regarding these dark rays, or obscure rays, or invisible rays—all these adjectives have been applied to them. We have defined light as wave motion; we have learned that the different colours of light are due to waves of different lengths; and we have also learned that side by side with the visible rays emitted by luminous sources, we have an outflow of invisible rays. This, accurately expressed, means that together with those waves which cross the

humours of the eye, impinge upon the retina, and excite the sense of vision, there are others which either do not reach the retina at all, or which, if they do, are not gifted with the power of producing that specific motion in the optic nerve which results in vision. Whether, and in what degree, the dark rays of the electric light reach the retina, shall be decided subsequently; but no matter what may be the cause of their inefficacy, whether it be due to their being quenched in the humours of the eye, or to a specific incompetence on their part to arouse the retina, all rays which fail to excite vision are called dark, obscure, or invisible rays; while all rays that can excite vision are called visible or luminous rays.

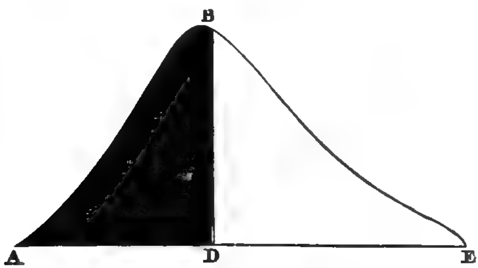
It must be confessed that there is a defect in the terms employed; for we cannot see light. In interstellar space we should be plunged in darkness, though the waves from all suns and all stars might be speeding through it. We should see the suns and the stars themselves, but the moment we turned our backs upon a star, its light would become darkness, though the ether all around us might be agitated by its waves. We cannot see the ether or its motions, and hence, strictly speaking, it is a misuse of language to speak of its waves or rays being visible or invisible. This form of expression, however, has taken root; its convenience has brought it into general use, and, understanding by the terms visible and invisible rays, wave motions which are respectively competent and incompetent to excite the optic nerve, no harm can result from the employment of the terms.

To the detection of those dark rays in the emission of the sun reference has been already made, and their existence in the emission of that source which comes next to the sun in power—the electric light—has also been demonstrated before you. The discoverer of the dark rays of the sun was, as you have been already informed, Sir

William Herschel. His means of observation were far less perfect than those now at our command; but, like Newton, he could extract from nature great results with very poor appliances. He caused thermometers to pass through the various colours of the solar spectrum, and noted the temperature corresponding to each colour. He pushed his thermometers beyond the extreme red of the spectrum, and found that the radiation, so far from terminating with the visible spectrum, rose to its maximum energy beyond the red. The experiment proved that side by side with its luminous rays the sun emitted others of lower refrangibility, which, although they possessed high calorific power, were incompetent to excite the sense of vision.

The rise of the thermometric column, when the instrument is placed in any colour of the spectrum, may

Fig. 111.

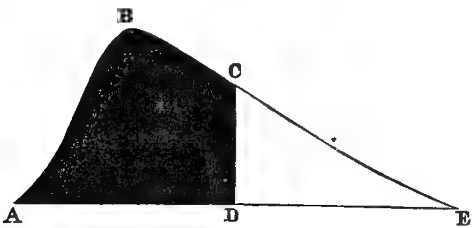


be represented by a straight line. For example, if a line of a certain length be taken to represent a rise of one degree, a line of twice that length will represent a rise of two degrees, while a line of half the length would represent a rise of half a degree. In order to show the distribution of heat in the spectrum of the sun, Sir William Herschel adopted this device of representing temperatures by lines. Drawing a horizontal line A E, fig. 111, to represent the length of the spectrum, visible and invisible,

and erecting at its various points perpendiculars to represent the heat of the spectrum, on uniting the ends of those perpendiculars, he obtained a curve, which exhibited at a glance the distribution of heat in the spectrum. The letter E marks a point in the blue of the spectrum where the heat first became sensible; from E to D, which marks the limit of the red, the temperature steadily increased, as shown by the increased height of the curve. At D the visible spectrum ceased, but an invisible one extended beyond D to A, where it vanished. According, then, to the observations of Sir William Herschel, the white space B D E represents the thermal value of the visible, while the black space A B D represents the thermal value of the invisible radiation of the sun.

With the more perfect apparatus subsequently devised by Melloni, Professor Müller of Freiburg examined

FIG. 112.



the distribution of heat in the solar spectrum. The results of his observations are rendered graphically in fig. 112, where the area D C E represents the visible, and A B C D the invisible radiation.

Before proceeding to our own measurements, it is desirable to make a few remarks upon the generation and intensification of rays, visible and invisible. A solid body at the ordinary temperature of our air has its molecules in motion: but it emits rays of too low a refrangibility, or,

in other words, it generates undulations which are too long and of too slow recurrence to excite vision. Conceive its temperature gradually augmented. With the increased temperature quicker vibrations are introduced; and at a certain temperature the vibrations are sufficiently rapid to affect the eye as light. The body glows, first, as already stated, with a red light; afterwards, as the temperature heightens, orange, yellow, green and blue are introduced in succession.

The vibrations corresponding to these successive colours are essentially new vibrations. But, simultaneously with the introduction of each new and more rapid vibration, we have an intensification of all the vibrations which preceded it. The vibrations executed when our ball was at the temperature of the air, continue to be executed when the ball is white-hot. But while the period remains thus constant, the amplitude, on which the intensity of the radiation depends, is enormously increased. For this reason, the rays emitted by an obscure body can never approach the intensity of the obscure rays emitted by a highly luminous one.

Let me rivet this subject upon your attention by a numerical example of the rise in the intensity of a special vibration, while more rapid ones are being introduced. A spiral of platinum wire was placed in a camera, and in front of the camera was placed a slit. A voltaic current was sent through the spiral, but not in sufficient strength to make it glow. By means of lenses and prisms of pure rock-salt, and by other suitable devices, an invisible spectrum of the rays emitted by the platinum-wire was obtained. A thin slice of this spectrum was permitted to fall upon the face of the linear thermo-pile already described. The band of the spectrum was so narrow and the radiation so weak, that the deflection of the galvanometer was in the first instance only one degree. Without

altering the position of any portion of the apparatus, the current was gradually strengthened; raising the temperature of the wire, from darkness through visible red, to an intense white heat. When this was reached a brilliant spectrum was projected on the screen to which the pile was attached, but the pile itself was outside the spectrum. It received invisible rays alone, and throughout the experiment it continued to receive those particular vibrations which first affected it. The amount of refraction, or in other words the rate of vibration, being determined by the position of the pile, as this position remained throughout unchanged, the vibration was unchanged also.

The following column of numbers shows the rise of intensity of the particular rays falling on the pile, when the platinum spiral passed through its various degrees of incandescence up to white heat:—

Appearance of spiral	Radiation of obscure band
Dark	1
Dark	6
Faint red	10
Dull red	13
Red	18
Full red	27
Orange	60
Yellow	93
Full white	122

Thus we prove that as the new and more rapid vibrations are introduced, the old ones become more intense, until at a white heat the obscure rays of a special refrangibility reach an intensity 122 times that possessed by them at the commencement. This abiding and augmentation of the dark rays when the bright ones are introduced may be expressed by the phrase *persistence of rays.*

THE ELECTRIC SPECTRUM.

What has been here demonstrated regarding an incandescent platinum spiral is also true of the electric

light. Side by side with an outflow of intensely luminous rays, we have a corresponding outflow of obscure ones. The carbon-points, like the platinum spiral, may be raised from a state of obscure warmth to a brilliancy almost equal to that of the sun, and as this occurs, the obscure radiation rises enormously in intensity. The accurate investigation of the distribution of heat in the spectrum of the electric light will fitly prepare the way for those experiments on invisible rays to which I shall subsequently direct your attention.

The thermo-pile employed is the beautiful instrument already referred to as constructed by Ruhmkorff, and represented in fig. 87, p. 278. It consists, as you know, of a single row of elements properly mounted and attached to a double brass screen. It has in front two silvered edges, which, by means of a screw, can be caused to close upon the pile, so as to render its face as narrow as desirable, reducing it to the width of the finest hair, or, indeed, shutting it off altogether. By means of a small handle and long screw, the plate of brass and the pile attached to it can be moved gently to and fro, and thus the vertical slit can be caused to traverse the entire spectrum, and to pass beyond it in both directions. The width of the spectrum was in each case equal to the length of the face of the pile.

To produce a steady spectrum of the electric light, I employed a regulator devised by M. Foucault and constructed by Duboscq, the constancy of which is admirable. A complete rock-salt train was employed. In the front orifice of the camera was placed a double lens of transparent rock-salt, intended to reduce to parallelism the divergent rays proceeding from the carbon-points. The parallel beam was permitted to pass through a narrow slit, in front of which was placed another rock-salt lens, the position of this lens being so arranged that a sharply-

defined image of the slit was obtained at a distance beyond it equal to that at which the spectrum was to be formed. Immediately behind this lens was placed a pure rock-salt prism (frequently two of them). The beam was thus decomposed, a brilliant horizontal spectrum being cast upon the screen which bore the thermo-electric pile. By turning the handle already referred to, the face of the pile could be caused to traverse the spectrum, an extremely narrow band of light, or of radiant heat, falling upon it at each point of its march.[1] A sensitive galvanometer was connected with the pile, and from its deflection the heating-power of every part of the spectrum, visible and invisible, was inferred.

Two modes of moving the instrument were practised, the description of one of which will be sufficient here. The face of the pile was brought to the violet end of the spectrum, where the heat is insensible, and moved through all the colours to the red; then past the red up to the position of maximum heat, and afterwards beyond this position until the heat of the invisible spectrum gradually faded away. The following table contains a series of measurements executed in this manner. The motion of the pile is expressed in turns of its handle, every turn corresponding to the shifting of the face of the instrument through a space of one millimetre, or $\frac{1}{25}$th of an inch. At the beginning, where the increment of heat was slow and gradual, the readings were taken at every two turns of the handle; on quitting the red, where the heat suddenly increases, the intervals were only half a turn, while near the maximum, where the changes were most sudden, the intervals were reduced to a quarter of a turn, which corresponded to a translation of the pile through $\frac{1}{100}$th of an inch. Intervals of one and of two turns were after-

[1] The width of the linear pile was 0·03 inch.

wards resumed until the heating-power ceased to be distinct. At every halting-place the deflection of the needle was noted. Calling the maximum effect in each series of experiments 100, the column of figures in the following table expresses the heat of all the other parts of the spectrum:—

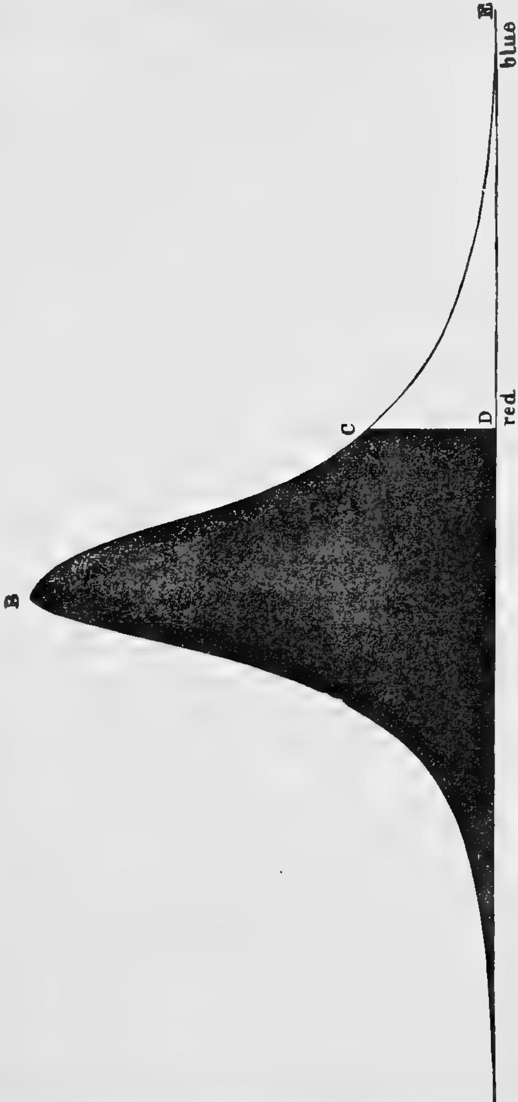
DISTRIBUTION OF HEAT IN SPECTRUM OF ELECTRIC LIGHT.

Movement of pile	Calorific intensity
Before starting (pile in the blue) . .	0
Two turns forward (green entered) .	2
,, ...	5
,, ...	8
,, (red entered) .	21
,, (extreme red) .	45
Half turn forward	60
,, ...	74
,, ...	85
,, ...	96
,, ...	99
Quarter turn forward, *maximum* . .	100
,, ...	97
Half turn forward	78
,, ...	62
,, ...	45
,, ...	36
Two turns forward	14
,, ...	9
,, ...	7
,, ...	5
,, ...	3
,, ...	2
,, ...	2

Here, as already stated, we begin in the blue and pass first through the visible spectrum. Quitting this at the place marked 'extreme red,' we enter the invisible calorific spectrum and reach the position of maximum heat, from which, onwards, the thermal power falls till it practically disappears.

More than a dozen series of such measurements were

Fig. 118.



Thermal Spectrum of Electric Light.

executed, each series giving its own curve. On superposing all the curves a very close agreement was found to exist between them. The annexed figure (fig. 113), which is the mean of several, expresses, with a near approximation to accuracy, the distribution of heat in the spectrum of the electric light from fifty cells of Grove. The space A B C D represents the invisible, while C D E represents the visible radiation. We here see the gradual augmentation of thermal power, from the blue end of the spectrum to the red. But in the region of dark rays beyond the red, the curve shoots suddenly upwards in a steep and massive peak—a kind of Matterhorn of heat—which quite dwarfs by its magnitude the portion of the diagram representing the visible radiation.

It is shown by this diagram that the ratio of the invisible to the visible radiation in the case of the sun is far less than in the case of the electric light. Fig. 112 shows the invisible radiation of the sun to be about twice the visible, while fig. 113 shows the invisible radiation of the electric light to be nearly eight times the visible. If we cause the beam from the electric lamp to pass through a layer of water of suitable thickness, we place its radiation in approximately the same condition as that of the sun; and on decomposing the beam after it has been thus sifted, we obtain a distribution of heat closely resembling that observed in the solar spectrum.

The curve representing the distribution of heat in the electric spectrum falls most steeply on that side of the maximum which is most distant from the red. On both sides, however, we have a *continuous* falling off. I have made numerous experiments to ascertain whether there is any interruption of continuity in the calorific spectrum; but all the measurements hitherto executed with artificial sources reveal a gradual and continuous augmentation of

heat from the point where it first becomes sensible up to the maximum.

Sir John Herschel has shown that this is not the case with the radiation from the sun when analysed by a flint-glass prism. Permitting the solar spectrum to fall upon a sheet of blackened paper, over which had been spread a wash of alcohol, this eminent philosopher determined by its drying-power the heating-power of the spectrum. He found that the wet surface dried in a series of spots, representing thermal maxima separated from each other by spaces of comparatively feeble calorific intensity. The subject was examined by M. Lamansky in 1871, and he verified and extended the observation of Sir John Herschel. No such maxima and minima were observed in the spectrum of the electric light, nor in the spectrum of a platinum wire raised to a white heat by a voltaic current. Prisms and lenses of rock-salt, of crown glass, and of flint glass were employed in these cases. In other experiments the beam intended for analysis was caused to pass through layers of water and other liquids of various thicknesses. Gases and vapours of various kinds were also introduced into the path of the beam. In all cases there was a general lowering of the calorific power, but the descent of the curve on both sides of the maximum seemed unbroken.

The rays from an obscure source cannot, as already remarked, compete in point of intensity with the obscure rays of a luminous source. No body heated under incandescence could emit rays of an intensity comparable to the obscure rays of the maximum region of the electric spectrum. If, therefore, we wish to produce intense calorific effects by invisible rays, we must choose those emitted by an intensely luminous source. The question then arises, how are the invisible calorific rays to be isolated from the visible ones?

The interposition of an opaque screen suffices to cut

off the visible spectrum of the electric light, and leaves us the invisible calorific rays to operate upon at our pleasure. Sir William Herschel experimented thus when he sought, by concentrating them, to render the invisible rays of the sun visible. But to form a spectrum in which the invisible rays shall be completely separated from the visible ones, a narrow slit is necessary; and this circumstance renders the amount of heat separable by prismatic analysis very limited. If we wish to ascertain what the intensely concentrated invisible rays can accomplish, we must devise some other mode of detaching them in mass from their visible companions. We must, in fact, discover a substance which shall filter the composite radiation of a luminous source by stopping the visible rays and allowing the invisible ones free transmission.

DIATHERMANCY OF IODINE; FILTRATION OF ELECTRIC BEAM.

The main object of these researches was, as already intimated, to make radiant heat an explorer of molecular condition, and the marked difference between elementary and compound bodies which the experiments reveal is, in my estimation, a point destined to be fruitful in important consequences. After this difference had come so clearly out in the deportment of gases, liquids were looked to, and the action of such as I was able to examine fell in surprisingly with the previously observed deportment of gaseous bodies. Could we obtain a *black* elementary body thoroughly homogeneous, and with all its parts in perfect optical contact, we should probably find it an effectual filter for the radiation of the sun or of the electric light. While cutting off the visible radiation, the black element would, probably, allow the invisible to pass.

Thus I reasoned. Now carbon in the state of soot is black, but its parts are not optically continuous. In

black glass the continuity is far more perfect, and hence the result, established by Melloni, that black glass possesses a very sensible power of transmission. Gold in ruby glass, or in the state of jelly prepared by Mr. Faraday, is exceedingly transparent to the invisible calorific rays, but it is not opaque enough to quench entirely the visible ones. The densely-brown liquid bromine is better suited to our purpose; for, in thicknesses sufficient to quench the light of our brightest flames, this element displays extraordinary diathermancy. Iodine cannot be applied in the solid condition, but it dissolves freely in various liquids, the solution in some cases being intensely dark. Here, however, the action of the element may be masked by that of its solvent. Iodine, for example, dissolves freely in alcohol; but alcohol is so destructive of the ultra-red rays, that it would be entirely unfit for experiments the object of which is to retain these rays, while quenching the visible ones. The same remark applies in a greater or less degree to many other solvents of iodine.

The deportment of bisulphide of carbon, both as a vapour and a liquid, suggested the thought that it would form a most suitable solvent. It is extremely diathermic, and there is hardly another substance able to hold so large a quantity of iodine in solution. Experiments already recorded prove that, of the rays emitted by a red-hot platinum spiral, 94·5 per cent. are transmitted by a layer of the liquid 0·02 of an inch in thickness, the transmission through the layers 0·07 and 0·27 of an inch thick being 87·5 and 82·5 respectively. Another experiment with a layer of greater thickness will exhibit the deportment of the transparent bisulphide towards the far more intense radiation of the electric light.

A cylindrical cell, 2 inches in length and 2·8 inches in diameter, with its ends stopped by plates of perfectly transparent rock-salt, was placed empty in front of an

electric lamp; the radiation from the lamp, after having crossed the cell, fell upon a thermo-pile, and produced a deflection of

73°.

Leaving the cell undisturbed, the transparent bisulphide of carbon was poured into it: the deflection fell to

72°.

A repetition of the experiment gave the following results:—

	Deflection
Through empty cell	74°
Through bisulphide	73

Taking the values of these deflections from a table of calibration and calculating the transmission, that through the empty cell being 100, we obtain the following results:—

	Transmission
From the first experiment . . .	94·9 per cent.
From the second experiment. . .	94·6 „
Mean . .	94·8

Hence the introduction of the bisulphide lowers the transmission only from 100 to 94·8.[1]

A *perfect* solvent of the iodine would be entirely neutral to the total radiation; and the bisulphide of carbon is shown by the foregoing experiment to approach very near perfection. We have in it a body capable of transmitting with little loss the total radiation of the electric light. Our object is now to filter this total, by the introduction into the bisulphide of a substance competent to quench the visible and transmit the invisible rays. That iodine does this with marvellous sharpness it is now my business to prove.

[1] The diminution of the reflection from the sides of the cell by the introduction of the bisulphide is not here taken into account.

A rock-salt cell, filled with the *transparent* bisulphide of carbon, was placed in front of the camera which contained the white-hot platinum spiral. The transparent liquid was then drawn off and its place supplied by the solution of iodine. The deflections observed in the respective cases are as follows :—

RADIATION FROM WHITE-HOT PLATINUM.

Through transparent liquid	Through opaque liquid
73·9°	73·8°
73·0	72·9

All the luminous rays passed through the transparent bisulphide, *none* of them passed through the solution of iodine. Still we see what a small effect is produced by their withdrawal. The actual proportion of luminous to obscure rays, as calculated from the above observations, may be thus expressed :—

Dividing the radiation from a platinum wire raised to intense whiteness by an electric current into twenty-four equal parts, one of those parts is luminous, and twenty-three obscure.

A bright gas-flame was substituted for the platinum spiral, the top and bottom of the flame being shut off, and its most brilliant portion chosen as the source of heat. The result of forty experiments with this source may be thus expressed :—

Dividing the radiation from the most brilliant portion of a flame of coal gas into twenty-five equal parts, one of those parts is luminous and twenty-four obscure.

I next examined the ratio of obscure to luminous rays in the electric light. A battery of fifty cells was employed and the rock-salt lens was used to render the rays from the carbon-points parallel. To prevent the deflection from reaching an inconvenient magnitude, the parallel rays were caused to pass through a circular aperture 0·1 of an inch in diameter, and were sent alternately through the

transparent bisulphide and the opaque solution. It is not easy to obtain perfect steadiness on the part of the electric light; but three experiments carefully executed gave the following deflections:

RADIATION FROM ELECTRIC LIGHT.

	Through transparent CS^2	Through opaque solution
Experiment No. I.	72·0°	70·0°
Experiment No. II. . . .	76·5	75·0
Experiment No. III. . . .	77·5	76·5

Calculating from these measurements the proportion of luminous to obscure heat, the result may be thus expressed:—

Dividing the radiation from the electric light produced by a Grove's battery of fifty cells, into ten equal parts, one of those parts is luminous and nine obscure.

The results hitherto obtained with various sources, radiating through iodine, may be thus tabulated:—

RADIATION THROUGH DISSOLVED IODINE.

Source	Absorption	Transmission
Dark spiral	0	100
Lampblack at 212° Fahr. .	0	100
Red-hot spiral . . .	0	100
Hydrogen-flame . . .	0	100
Oil-flame.	3	97
Gas-flame	4	96
White-hot spiral . . .	4·6	95·4
Electric light	10	90

Subsequent experiments with a battery of fifty cells made the transmission in the case of the electric light 89, and the absorption 11. Considering the transparency of the iodine for heat emitted by all the sources under, or barely up to incandescence, it may be inferred that the absorption of 11 per cent. represents the calorific intensity of the *luminous rays* alone. By the method of filtering, therefore, we make the invisible radiation of the electric

light about eight times the visible. Computing, by means of a proper scale, the area of the spaces A B C D, C D E (fig. 113), the former, which represents the invisible emission, is found to be 7·7 times the latter. Prismatic analysis, therefore, and the method of filtering yield almost exactly the same result.

It is plain from the description of the experiments that the foregoing results refer to the action of the iodine dissolved in the bisulphide of carbon. The transmission of 100, for example, does not indicate that the solution itself, but that the iodine in the solution, is perfectly diathermic to the radiation from the first four sources.

ACTION OF INVISIBLE RAYS DETACHED BY FILTRATION.

Having thus, in the solution of iodine, found a means of almost perfectly detaching the obscure from the luminous rays of the electric light, we are able to operate at will upon the former. I place a rock-salt lens in the camera and focus it so as to form a very small image of the carbon-points. A battery of fifty cells being employed, the track of the cone of rays emergent from the lamp is plainly seen in the air, and their point of convergence therefore easily fixed. Fixing the cell containing the opaque solution in front of the lamp, the luminous cone is entirely cut off, but the intolerable temperature of the focus, when the hand is placed there, shows that the calorific rays are still transmitted. Placing successively in the dark focus thin plates of tin and zinc, they are speedily fused; matches are ignited, gun-cotton is exploded, and brown paper set on fire. With a battery of sixty of Grove's cells, all these results are readily obtained with the ordinary glass lenses of Duboscq's electric lamp. It is extremely interesting to observe in the air of a perfectly dark room a piece of black paper suddenly

pierced by the invisible rays, and the burning ring expanding on all sides from the centre of ignition.[1]

From the setting of paper on fire and the fusion of non-refractory metals to the rendering of refractory bodies incandescent by the invisible rays, the step was immediate and inevitable. And here the inquiry derived a stimulus from the fact, that on theoretic grounds some eminent men doubted whether incandescence by invisible rays was possible. A moment's reflection will make plain to you that the success of the experiment involved *a change of period* on the part of the calorific waves. For if without the aid of combustion, waves of too slow a recurrence to excite the sense of vision were to render a refractory body luminous, it could only be by compelling the molecules of that body to vibrate more rapidly than the waves which fell upon them. Whether this change of period could be effected had been for a long time considered doubtful.

A few preliminary experiments with platinum-foil, which resulted in failure, raised the question whether, even with the *total radiation* of the electric light, it would be possible to obtain incandescence without combustion. Abandoning the use of lenses altogether, a thin leaf of platinum was caused to approach the ignited carbon-points. It was observed by myself from behind, while my assistant stood beside the lamp, and, looking through a dark glass, marked the distance between the platinum-foil and the electric light. At half an inch from the carbon-points the metal became red-hot. The problem now before me was to obtain, at a greater distance, a focus of rays which should possess a heating-power equal to that of the direct rays at a distance of half an inch.

[1] The intensity of the invisible foci obtained by filtering the electric beam by dissolved iodine was first shown in my lectures in the spring of 1862, and very frequently afterwards.

In the first attempt the direct rays were utilised as much as possible. A piece of platinum-foil was placed at a distance of an inch from the carbon-points, there receiving the direct radiation. The rays emitted *backwards* from the points were at the same time converged by a small mirror upon the foil, and were found more than sufficient to compensate for the diminution of intensity due to withdrawal of the foil to the distance of an inch. By the same method, incandescence was subsequently obtained when the foil was removed to a distance of two and three inches from the carbon-points.

This latter interval enabled me to introduce between the focus and the source of rays a cell containing a solution of iodine. The dark rays transmitted were found sufficient to *inflame* paper, and to raise platinum-foil to incandescence.

The experiments, however, were not unattended with danger. The bisulphide of carbon is extremely inflammable; and on the 2nd of November, 1864, while employing a very powerful battery and intensely-heated carbon-points, the substance took fire, and instantly enveloped the electric lamp and all its appurtenances in flame. Happily the precaution had been taken of placing the entire apparatus in a flat vessel containing water, into which the flaming mass was summarily overturned. The bisulphide of carbon being heavier than the water, sank to the bottom, so that the flames were speedily extinguished. Similar accidents occurred twice subsequently.

Such occurrences caused me to seek earnestly for a substitute for the bisulphide. Pure chloroform, though not so diathermic, transmits the invisible rays pretty copiously, and it freely dissolves iodine. In layers of the thickness employed, however, the solution was not sufficiently opaque; and its absorptive power enfeebled the effects. The same remark applies to the iodides of me-

thyl and ethyl, to benzol, acetic ether, and other substances. They all dissolve iodine, but they weaken the results by their action on the dark rays.

Special cells were then constructed with a view to employing the element bromine and chloride of sulphur. Neither of these substances is inflammable; but they are both intensely corrosive, and their action upon the lungs and eyes was so irritating as to render their employment impracticable. With both liquids, however, powerful effects were obtained; still their diathermancy did not come up to that of the dissolved iodine. Bichloride of carbon would be invaluable if its solvent power were equal to that of the bisulphide. It is not at all inflammable, and its own diathermancy appears equal to that of the bisulphide. But in reasonable thicknesses the iodine which it can dissolve is not sufficient to render the filter perfectly opaque. The solution forms a purple colour of exquisite beauty; and though unsuited to strict crucial experiments on dark rays, this filter may be employed with excellent effect in class experiments.

Thus foiled in my attempts to obtain a solvent equally good as, and less dangerous than, the bisulphide of carbon, I sought to reduce the danger of employing it to a minimum. A tin camera was constructed, within which were placed both the lamp and its converging mirror. Through an aperture in front, $2\frac{3}{4}$ inches wide, the cone of reflected rays issued, forming a focus outside the camera. Underneath this aperture was riveted a stage, on which the solution of iodine rested, thus closing the aperture and cutting off all the light. At first nothing intervened between the cell and the carbon-points; but the peril of thus exposing the bisulphide caused me to make the following changes. A perfectly transparent plate of rock-salt, secured in a proper cap, was employed to close the aperture; and by it all direct communication between the solution

and the incandescent carbons was cut off.[1] To keep the camera cool the aperture was surrounded by an annular space, about $2\frac{1}{2}$ inches wide and a quarter of an inch deep, through which cold water was caused to circulate. The cell containing the solution was surrounded by a jacket, and the current of water having completed its course round the aperture, passed round the cell. Thus the apparatus was kept cold. The neck of the cell was stopped by a closely-fitting cork; through this passed a piece of glass tubing, which, when the cell was placed upon its stage, ended at a considerable height above the focus. Experiments on combustion might therefore be carried on at the focus without fear of igniting the vapour which, even under the improved conditions, might escape from the bisulphide of carbon.

The arrangement will be at once understood by reference to figs. 114 and 115, which show the camera, lamp, and filter both from the side and from the front. x y, fig. 114, is the mirror, from which the reflected cone of rays passes, first, through the rock-salt window, and afterwards through the iodine filter m n. The rays converge to the focus k, where they would form an invisible image of the lower carbon-point; the image of the upper would be thrown below k. *Both images spring vividly forth when a leaf of platinised platinum is exposed at the focus.* At s s, figs. 114 and 115, is shown, in section, and in plan, the annular space in which the cold water circulates.

With this arrangement, and a battery of fifty cells, the following results were obtained :—

A piece of silver leaf, tarnished by exposure to the fumes of sulphide of ammonium, and fastened to a wire ring, when held in the dark focus, was raised to vivid redness.

[1] With care it is possible to work securely without this plate of rock-salt. I never make use of it now.

Copper-leaf tarnished in a similar manner, when placed at the focus, was raised to redness.

A piece of platinised platinum-foil was supported in an exhausted receiver, the vessel being so placed that the focus of invisible rays fell upon the platinum. The heat of the focus was instantly converted into light, a

Fig. 114.

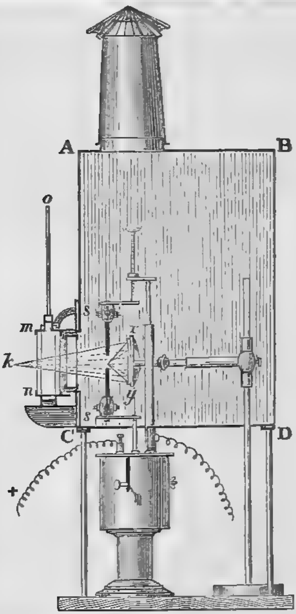
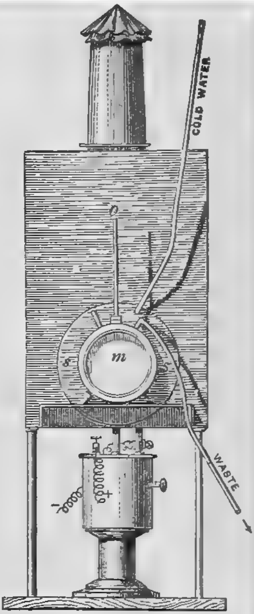


Fig. 115.



clearly-defined image of the points being stamped upon the metal. Fig. 116 represents the thermograph of the carbons.

Blackened paper was now substituted for the platinum in the exhausted receiver. Placed at the focus of invisible rays, the paper was instantly pierced, and a cloud of smoke fell like a cascade to the bottom of the receiver. The paper seemed to burn without incandescence. Here

also a thermograph of the carbon-points was stamped out. When black paper is placed at the focus, the thermal image being well defined, it is always pierced in two points, answering to the images of the two carbons. The superior heat of the positive carbon is shown by the fact that its rays first pierce the paper, burning out a large space and showing its peculiar crater-like top, while the negative carbon usually pierces a small hole.

FIG. 116.

Paper reddened by the iodide of mercury had its colour discharged at the places on which the invisible image of the carbon-points fell upon it, though not with the expected promptness.

Disks of paper carbonised by different processes were raised to brilliant incandescence, both in the air and in an exhausted receiver.

In these earlier experiments an apparatus was employed which had been constructed for other purposes. The mirror, for example, was one detached from a Duboscq's camera; it was first silvered at the back, but afterwards silvered in front. The cell employed for the iodine solution was also that which usually accompanies Duboscq's lamp, being intended by its maker for a solution of alum. Its sides are of good white glass, the width from side to side being 1·2 inch.

EXPOSURE OF THE EYE TO INVISIBLE RAYS.

A point of considerable theoretic importance was involved in these experiments. In his excellent researches on fluorescence, Professor Stokes had invariably found the refrangibility of the incident light to be *lowered*. This rule was so constant as almost to enforce the conviction that it was a law of nature. But if the rays which in the foregoing experiments raised platinum and gold and silver to a red heat were wholly ultra-red, the rendering visible of the metallic films would be an instance of *raised* refrangibility.

And here I thought it desirable to make sure that no trace of visible radiation passed through the solution, and also that the invisible radiation was exclusively ultra-red.

This latter condition might seem to be unnecessary, because the calorific action of the ultra-violet rays is so exceedingly feeble (in fact it is immeasurably small) that, even supposing them to reach the platinum, their heating power would be an utterly vanishing quantity. Still the exclusion of *all* rays of high refrangibility was necessary to the complete solution of the problem. Hence, though the iodine employed in the foregoing experiments was sufficient to cut off the light of the sun at noon, I wished to submit its opacity to a severer test. The following experiments were accordingly executed.

The rays from the electric lamp being duly converged by the mirror, the iodine-cell was placed in the path of the convergent beam, its light being thereby to all appearance totally intercepted. With a piece of platinum-foil the focus was found and marked, and a cell containing a solution of alum was then placed between the focus and the iodine-cell. The alum solution diminished materially

the invisible radiation, but it was without sensible influence upon the visible rays.

All stray light issuing from the crevices in the lamp being cut off, and the daylight also being excluded from the room, the eye was caused slowly to approach the focus. On reaching it, a singular appearance presented itself. The incandescent carbon-points of the lamp were seen black, projected on a deep-red ground. Their motions could be followed, and when brought into contact, a white space was seen at the extremities of the points, appearing to separate them. The points were seen erect. By careful observation the whole of the points could be observed, and even the holders which supported them. The black appearance of the incandescent portion of the points was here only *relative*; they appeared dark because they intercepted more of the light reflected from the mirror behind than they could make good by their direct emission.

The solution of iodine, 1·2 inch in thickness, proving thus unequal to the test applied to it, I had two other cells constructed—the one with transparent rock-salt sides, the other with glass ones. The width of the former was 2 inches, that of the latter nearly 2½ inches. Filled with the solution of iodine, these cells were placed in succession in front of the camera, and the concentrated beam was sent through them. Determining the focus as before, and afterwards introducing the alum-cell, the eye on being brought up to the focus received no impression of light.

The alum-cell was then abandoned, and the undefended eye was caused to approach the focus: the heat was intolerable, but it seemed to affect the eyelids and not the eye itself. An aperture somewhat larger than the pupil being made in a metal screen, the eye was placed behind it, and brought slowly and cautiously up to the focus.

The concentrated beam entered the pupil; but no impression of light was produced, nor was the retina sensibly affected by the heat. The eye was then withdrawn, and a plate of platinised platinum was placed in the position occupied by the retina a moment before. It instantly rose to vivid redness.[1] The failure to obtain, with the most sensitive media, the slightest evidence of fluorescence at the obscure focus, proved the invisible rays to be exclusively ultra-red. It will be subsequently shown that a considerable portion of these rays actually reached the retina.

TRANSMUTATION OF RAYS, CALORESCENCE.

When intense effects are sought, we collect as many of the invisible rays as possible, and concentrate them on the smallest possible space. The nearer the mirror is to the source of rays, the more of these rays will it intercept and reflect, and the nearer the focus is to the same source, the smaller will the image be. To secure proximity both of focus and mirror, the latter must be of short focal length. If a mirror of long focal length be employed, its distance from the source of rays must be considerable to bring the focus near the source, but when placed thus at a distance, a great number of rays escape the mirror altogether. If, on the other hand, the mirror be too deep, spherical aberration comes into play; and though a vast quantity of rays may be collected, their convergence at the focus is imperfect. To determine the best form of mirror, three were constructed: the first 4·1 inches in diameter, and of 1·4 inch focal length; the second 7·9 inches in diameter, and of 3 inches focal length; the third 9 inches in diameter, with a focal length of 6 inches. Fractures caused by imperfect annealing repeatedly occurred; but

[1] I do not recommend the repetition of these experiments.

at length I was so fortunate as to obtain the three mirrors each without a flaw. The most convenient distance of the focus from the source was found to be about 5 inches: and the position of the mirror ought to be arranged accordingly. This distance permits of the introduction of an iodine-cell of sufficient width, while the heat at the focus is exceedingly powerful.

And now with this improved apparatus I will run through my principal experiments on invisible heat-rays. The dense volumes of smoke which rise from a blackened block of wood when it is placed in the dark focus are very striking: matches are at once ignited, and gunpowder instantly exploded. Dry paper bursts into flame. Chips of wood are also inflamed: the dry wood of a hat-box is very suitable for this experiment. When a sheet of brown paper is placed a little beyond the focus, it is first brought to vivid incandescence over a large space; the paper then yields, and the combustion propagates itself as a burning ring round the centre of ignition. Charcoal is made an ember at the focus, and disks of charred paper glow with extreme vividness. When blackened zinc foil is placed at the focus it bursts into flame; and by slowly moving the foil about, its ignition may be kept up till the whole of it is consumed. Magnesium wire, flattened at the end and blackened, also bursts into vivid combustion. A cigar of course is instantly lighted. The bodies experimented on may be enclosed in glass receivers; the concentrated obscure rays will burn them after having crossed the glass. This glass jar, for example, contains oxygen; and in the oxygen by means of a suitable holder is plunged a bit of charcoal bark. When the dark rays are concentrated upon the charcoal it instantly throws out showers of scintillations.

In all these cases the body exposed to the action of the invisible rays was more or less combustible. It was first

heated and then exposed to the attack of oxygen. The vividness observed was in part due to combustion, and does not furnish a conclusive proof that the refrangibility of the incident rays was elevated. This, however, is effected by exposing non-combustible bodies at the focus, or by enclosing combustible ones in a space devoid of oxygen. Both in air and *in vacuo* platinised platinum-foil has been repeatedly raised to a white heat. The same result has been obtained with a sheet of charcoal or coke suspended *in vacuo*. Now the waves from which this light was extracted had neither the visible nor the ultra-violet rays commingled with them; they were exclusively ultra-red. The action, therefore, of the atoms of platinum, copper, silver, and carbon upon these rays transmutes them from heat-rays into light-rays. They impinge upon these atoms at a certain rate; they return from them at a quicker rate, the invisible being thus rendered visible.

On looking at the white-hot platinum through a prism of transparent bisulphide of carbon, a rich and vivid spectrum is seen to be extracted from the darkness.

To express this transmutation of heat-rays into others of higher refrangibility, I propose the term *calorescence*. It harmonises well with the term 'fluorescence' introduced by Professor Stokes, and is also suggestive of the character of the effects to which it is applied. The phrase 'transmutation of rays,' introduced by Professor Challis, covers both classes of effects.[1]

I have sought to fuse platinum with the invisible rays of the electric light, but hitherto without success. In some experiments a large model of Foucault's lamp was employed, with a battery of 100 cells. In other experiments two batteries were employed, one of 100 cells and one of 70, making use of two lamps, two mirrors, and two

[1] *Philosophical Magazine*, 1865. Vol. xxix. p. 386.

filters, and converging the heat of both lamps in opposite directions upon the same point. When a leaf of platinum was placed at the common focus, the converged beams struck it at opposite sides, and raised it to dazzling whiteness. I am persuaded that the metal could be fused, if the platinum-black upon its surface could be retained. But this was immediately dissipated by the intense heat, and the reflecting power of the metal coming into play, the absorption was so much lowered that fusion was not effected. By coating the platinum with lampblack it has been brought to the verge of fusion, the incipient yielding of the mass being perfectly apparent after it had cooled. Here, however, as in the case of platinised platinum, the absorbing substance disappears too quickly. Copper and aluminium, however, when thus treated, are speedily burnt up.

The thermal isolation of the luminiferous ether from the air is strikingly illustrated by these experiments. The air at the focus may be of a freezing temperature, while the ether possesses an amount of heat competent, if absorbed, to impart to that air the temperature of flame. An air-thermometer is unaffected where platinum is raised to a white heat.

EXPERIMENTAL ARRANGEMENTS.

Arrangements have already been described with a view of avoiding the danger incidental to the use of so inflammable a substance as the bisulphide of carbon. I have thought of accomplishing this end by simpler means, and thus facilitating the repetition of the experiments. The arrangement now before you, fig. 117, may be adopted with safety.

A B C D is an outline of the camera.

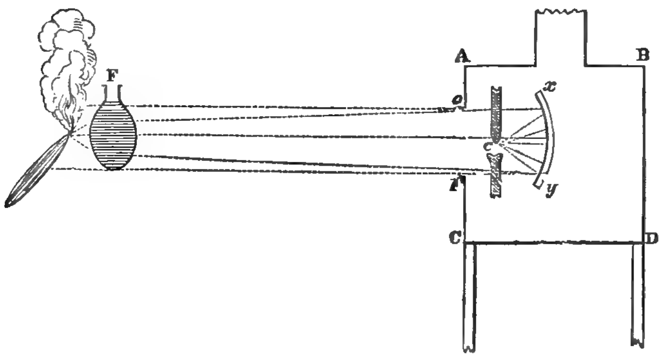
x y the silvered mirror within it.

c the carbon-points of the electric light.

o p the aperture in front of the camera, through which issues the beam reflected by the mirror *x y*.

Let the distance of the mirror from the carbon-points be such as to render the reflected beam slightly convergent. Fill a glass flask F with the solution of iodine, and place it in the path of the reflected beam at a safe distance from the lamp. The flask acts as a lens and filter at the same time, the light rays are intercepted, and the dark heat-rays are powerfully converged. At the focus formed a

FIG. 117.



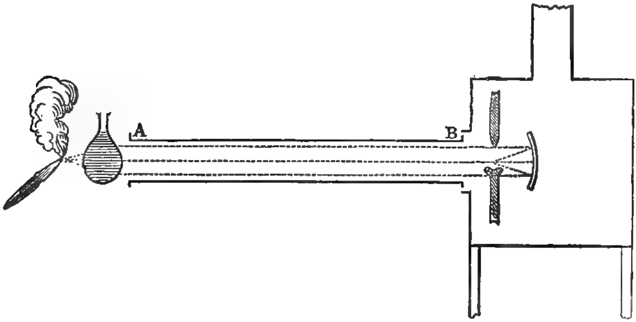
little beyond the flask combustion and calorescence may be produced. Flasks with diameters from $1\frac{1}{2}$ to 3 inches are well suited for the experiments.

By the arrangement here described, I have raised platinum to redness at a dark focus 22 feet distant from the source of the rays.

The best mirror, however, scatters the rays more or less; and by such scattering, the beam at a great distance from the lamp becomes much enfeebled. The effect is therefore intensified when the beam is caused to pass through a tube, polished within, which prevents the lateral

waste of radiant heat. Such a tube, placed in front of the camera, is represented at A B, fig. 118. The flask may be held against its end by the hand, or it may be permanently

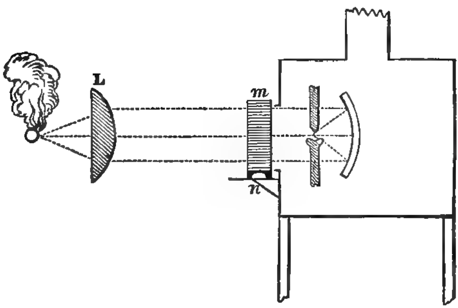
FIG. 118.



fixed there. With a battery of fifty cells, platinum may be raised to a white heat at the focus of the flask.

Again let a lens of glass or rock-salt (L, fig. 119), 2·5 inches wide, and having a focal length of 3 inches, be

FIG. 119.

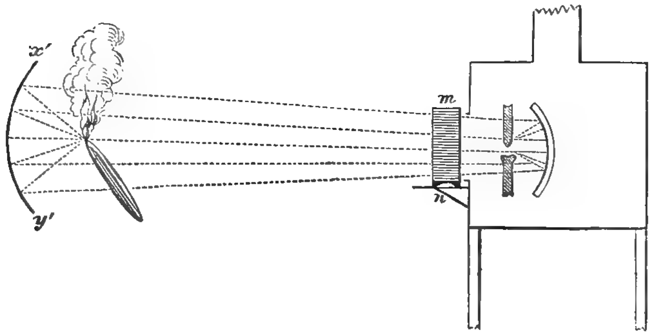


placed in the path of the reflected parallel beam. The rays are converged; and at their point of convergence all the effects of calorescence and combustion may be obtained, the

luminous rays being cut off by a cell *m n*, with plane glass sides and containing the opaque solution.

Finally, the arrangement shown in fig. 120 may be adopted. The beam reflected by the mirror within the camera is received and converged by a second mirror *x′ y′*. At the point of convergence, which may be several feet

FIG. 120.



from the camera, all the effects hitherto described may be obtained.[1]

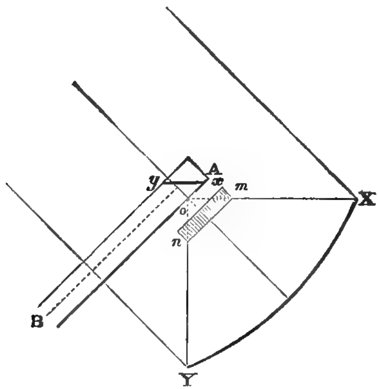
Thus far I have dealt exclusively with the invisible radiation of the electric light; but all solid bodies raised to incandescence emit these invisible calorific rays. The denser the incandescent body, moreover, the more powerful is its obscure radiation. We possess at the Royal Institution very dense cylinders of lime for the production of the Drummond light; and when a copious oxyhydrogen-flame is projected against one of them it shines with an intense yellowish light, while the obscure radiation is exceedingly powerful. Filtering the latter from the total emission by the solution of iodine, combustion and calor-

[1] I frequently employ this method of experiment. It is very effective.

escence may be obtained at the focus of the invisible rays. The light obtained by projecting the oxyhydrogen-flame upon compressed magnesia, after the manner of Signor Carlevaris, is whiter than that emitted by our lime; but the substance being light and spongy, its obscure radiation is surpassed by that of our more solid cylinders.

The invisible rays of the sun have also been transmuted. A concave mirror, three feet in diameter, was mounted on the roof of the Royal School of Mines in Jermyn Street. The focus was formed in a darkened

FIG. 121.



chamber, in which the platinised platinum-foil was exposed. Cutting off the visible rays by the solution of iodine, feeble but distinct incandescence was there produced by the invisible rays.

To obtain a clearer sky, this mirror was transferred to the garden of a friend near Chislehurst. A blackened tin tube (A B, fig. 121) with square cross section had one end open, while at the other was fixed a plane mirror (x y), forming an angle of 45° with the axis of the tube. A lateral aperture (x o), about two inches square, was cut out

in front of the mirror. Over this aperture was placed a leaf of platinised platinum. Turning the leaf towards the concave mirror, the concentrated sunbeams were permitted to fall upon it. In the full glare of daylight it was quite impossible to see whether the platinum was incandescent or not; but placing the eye at B, the glow of the platinum could be seen by reflection from the plane mirror. Incandescence was thus obtained at the focus of the large mirror, X, Y, after the removal of the visible rays by the iodine solution, *m n*.[1]

The effects obtained with the total solar radiation were extraordinary. Large spaces of the platinum-leaf, and even thick foil, when exposed at the focus, disappeared as if vaporised. The handle of a pitchfork, similarly exposed, was soon burnt quite across. Paper placed at the focus burst into flame with almost explosive suddenness. The high ratio which the visible radiation of the sun bears to the invisible was strikingly manifested in these experiments. With a *total* radiation vastly inferior, the invisible rays of the electric light, or of the lime-light, raised platinum to whiteness, while, when the visible constituents of the concentrated sunbeam were intercepted, the most that could be obtained from the dark rays was a bright red-heat. The heat of the solar luminous rays, moreover, is so great as to render it exceedingly difficult to experiment with the solution of iodine. It boiled up incessantly, exposure for two or three seconds being sufficient to raise it to ebullition. This high ratio of the luminous to the non-luminous radiation, is doubtless to be ascribed in part to the absorption of a large portion of the latter by the aqueous vapour of the air. From it, however, may also be inferred the enormous temperature of the sun.

Converging the sun's rays with a hollow lens filled with

[1] Experiments on the sun had been previously, but unsuccessfully, attempted by others.

the solution of iodine, incandescence was obtained at the invisible focus of the lens on the roof of the Royal Institution.

Knowing the permeability of good glass to the solar rays, I requested Mr. Mayall to permit me to make a few experiments with his fine photographic lens at Brighton. Though exceedingly busy at the time, he in the kindest manner abandoned to my assistant the use of his apparatus for the three best hours of a bright summer's day. A red heat was obtained at the focus of the lens after the complete withdrawal of the luminous portion of the radiation.

Black paper has been very frequently employed in the foregoing experiments, the action of the invisible rays upon it being most energetic. This suggests that the absorption of the dark rays is not independent of colour. A red powder is red because of the entrance and absorption of all luminous rays of higher refrangibility than the red, and the ejection of the unabsorbed red light by reflection at the limiting surfaces of the particles of the red body. This feebleness of absorption extends in many cases to the obscure rays beyond the red; and the consequence is that red paper when exposed at the focus of invisible rays is often scarcely charred, while black paper bursts in a moment into flame. The following table exhibits the condition of paper of various kinds when exposed at the dark focus of an electric light of moderate intensity:—

Paper.	Condition.
Glazed orange-coloured paper .	Barely charred.
„ red- „ .	Scarcely tinged; less than the orange.
„ green- „ .	Pierced with a small burning ring.
„ blue- „ .	The same as the last.
„ black- „ .	Pierced; and immediately set ablaze.
„ white- „ .	Charred; not pierced.
Thin foreign-post . .	Barely chared; less than the white.
Foolscap . . .	Still less charred; about the same as the orange.

Paper.	Condition.
Thin white blotting-paper	. Scarcely tinged.
„ whitey-brown „	. The same; a good deal of heat seems to get through these last two papers.
Ordinary brown „	. Pierced immediately, a beautiful burning ring expanding on all sides.
Thick brown „	Pierced, not so good as the last.
Thick white sand-paper .	. Pierced with a burning ring.
Brown emery „ .	The same as the last.
Dead-black „ .	Pierced, and immediately set ablaze.

We have here an almost total absence of absorption on the part of the red paper. Even white absorbs more, and is consequently more easily charred. Rubbing the red iodide of mercury over paper, and exposing the reddened surface at the focus, a thermograph of the carbon-points is obtained, which shows itself by the discharge of the colour at the place on which the invisible image falls. Expecting this change of colour to be immediate, I was surprised at the length of time necessary to produce it.

EXPERIMENT OF FRANKLIN.

And here we find ourselves in a position to properly qualify and explain a popular experiment which has been fruitful in erroneous inferences. The celebrated Dr. Franklin placed cloths of various colours upon snow and allowed the sun to shine upon them. They absorbed the solar rays in different degrees, became differently heated, and sank therefore to different depths. His conclusion was that dark colours were the best absorbers, and light colours the worst; and to this hour we appear to have been content to accept Franklin's generalisation without qualification. Did the emission from luminous sources consist exclusively of visible rays, we might fairly infer from the colour of a substance its capacity to absorb the heat of such sources. But we now know that the emission

from luminous sources is by no means all visible. In terrestrial sources by far the greater part, and in the case even of the sun a very great part, of the emission consists of invisible rays, regarding which colour teaches us nothing.

It remained therefore to examine whether the results of Franklin were the expression of a law of nature. Two cards were taken of the same size and texture; over one of them was shaken the white powder of alum, and over the other the dark powder of iodine. Placed before a glowing fire and permitted to assume the maximum temperature due to their position, it was found that the card bearing the alum became extremely hot, while that bearing the iodine remained cool. No thermometer was necessary to demonstrate this difference. Placing the back of the iodine card against the forehead or cheek, no inconvenience was experienced; while the back of the alum card similarly placed proved intolerably hot.

This result was corroborated by the following experiments:—One bulb of a differential thermometer was covered with iodine, and the other with alum powder. A red-hot spatula being placed midway between both, the liquid column associated with the alum-covered bulb was immediately forced down, and maintained in an inferior position.

Two delicate mercurial thermometers had their bulbs coated, the one with iodine, the other with alum. On exposing them at the same distance to the radiation from a gas-flame, the mercury of the alum-covered thermometer rose nearly twice as high as that of its neighbour.

Two sheets of tin were coated, the one with alum, and the other with iodine powder. The sheets were placed parallel to each other, and about 10 inches asunder; at the back of each was soldered a little bar of bismuth, which, with the tin plate to which it was attached, con-

stituted, as in fig. 95, p. 302, a thermo-electric couple. The two plates were connected together by a wire, and the free ends of the bismuth bars were connected with a galvanometer. Placing a red-hot ball midway between both, the calorific rays fell with the same intensity on the two sheets, but the galvanometer immediately declared that covered by the alum to be the most highly heated.

In some of the foregoing cases the iodine was simply shaken through a muslin sieve; in other cases it was mixed with bisulphide of carbon and applied with a camel's-hair brush. When dried afterwards it was almost as black as soot; but as an absorber of radiant heat it was no match for the perfectly white powder of alum.

This difficulty of warming iodine by radiant heat is evidently due to the diathermic property which it manifests so strikingly when dissolved in bisulphide of carbon. The heat enters the powder, is reflected at the limiting surfaces of the particles, but it does not lodge itself among the atoms of the iodine. When shaken in sufficient quantity on a plate of rock-salt and placed in the path of a calorific beam, iodine intercepts the heat. But its action is mainly that of a white powder on light; it is impervious, not through absorption, but through repeated internal reflection. Ordinary roll sulphur, even in thin cakes, allows no radiant heat to pass through it; but its opacity is also due to internal reflection. The temperature of ignition of sulphur is about 244° C.; but on placing a small piece of the substance at the obscure focus of the electric lamp, where the heat was sufficient to raise, in a moment, platinum-foil to whiteness, it required exposure for a considerable time to fuse and ignite the sulphur. Though impervious to the heat, it was not so through absorption Sugar is a much less inflammable substance than sulphur, but it is a far better absorber: exposed at the focus, it is

speedily fused and burnt up. The heat moreover which is competent to inflame powdered sugar, is scarcely competent to warm table-salt, of the same white appearance.

A fragment of almost black amorphous phosphorus exposed at the dark focus of the electric lamp, refuses, for a long time, to be ignited. A still more remarkable result was obtained with ordinary phosphorus. A small fragment of this exceedingly inflammable substance could be exposed for twenty seconds without ignition at a focus where platinum was almost instantaneously raised to a white heat. The fusing-point of phosphorus is about 44° C., that of sugar is 160°; still at the focus of the electric lamp the sugar fuses before the phosphorus. All this is due to the diathermancy of the phosphorus; a thin disk of the substance placed between two plates of rock-salt permits of a copious transmission. This substance therefore takes its place with other elementary bodies as regards its deportment towards radiant heat.

The surface of a vessel covered with a thick fur of hoar-frost was exposed to the beam of the electric lamp condensed by a powerful mirror, the beam having been previously sent through a cell containing water. The sifted beam was powerless to remove the frost, though it was competent to set wood on fire. We may largely apply this result. It is not, for example, the luminous rays, but the dark rays of the sun, which sweep the snows of winter from the slopes of the Alps. It is also the invisible solar rays which lift the glaciers from the sea-level to the summits of the mountains; for the luminous rays penetrate the tropical ocean to great depths, while the non-luminous ones are absorbed close to the surface, and become the main agents in evaporation.

We will end this subject by fulfilling a promise formerly made. The method by which Melloni determined

the ratio of the visible to the invisible rays emitted by any luminous source has been already described. It was explained to you, that assuming a solution of alum to transmit all the visible rays, which is sensibly the case, and to absorb all the invisible rays, the difference between the transmission through alum and rock-salt gives the action of the obscure rays. But is this assumption regarding the absorptive power of alum correct? Is a solution of this substance, of the thickness hitherto applied, really competent to absorb all heat-rays of a lower refrangibility than those which produce light?

The solution of iodine, with which you are now so intimately acquainted, was placed in front of an electric lamp, the luminous rays being thereby intercepted. Behind the rock-salt cell containing the opaque solution was placed a glass cell, empty in the first instance. The deflection produced by the obscure rays which passed through both produced a deflection of

80°.

The glass was now filled with a concentrated solution of alum; the deflection produced by the obscure rays passing through both solutions was

50°.

Calculating from the values of these deflections, it is found *that of the obscure heat emergent from the solution of iodine* 20 *per cent. was transmitted by the alum.*[1]

The question, whether the invisible rays emitted by luminous sources reach the retina of the eye, we have

[1] In passing from one medium to another, light is always reflected; the same is true of radiant heat. And in the case of our empty glass cell, radiant heat was reflected from its two interior surfaces when it was empty. The introduction of the alum solution no doubt altered the quantity of heat reflected; for the sake of simplicity, I have neglected taking this into account; my doing so would not materially affect the results here enunciated.

hitherto left in abeyance. But there cannot be a doubt that the invisible rays which have shown themselves competent to traverse such a thickness of the most opaque liquid yet discovered are also able to pass through the humours of the eye. Dr. Franz has indeed proved this to be the case for the dark solar rays. The very careful and interesting experiments of M. Janssen [1] prove, moreover, that the humours of the eye absorb an amount of radiant heat exactly equal to that absorbed by a layer of water of the same thickness; and in our solution the power of alum is added to that of water. Direct experiments on the vitreous humour of an ox lead me to conclude that nearly two-thirds of the whole radiant energy, visible and invisible, which the electric light sends to the retina is incompetent to excite vision.

Measured by a photometer the intensity of the electric light used by me was, in some cases, 1000 times that of a good composite candle; and as the non-luminous heat-rays from the carbon-points which reach the retina have, in round numbers, twice the energy of the luminous, it follows that at a common distance, say of a foot, the energy of the radiant heat which reaches the optic nerve, but is incompetent to provoke vision, is 2000 times that of the light of a candle. But on a tolerably clear night a candle-flame can be readily seen at the distance of a mile; and the intensity of the candle's light at this distance is less than one thirty-millionth of its intensity at the distance of a foot; hence the energy which renders the candle perfectly visible a mile off, would have to be multiplied by 2000 × 30,000,000, or by sixty thousand millions, to bring it up to the intensity of the radiation which the retina actually receives from the carbon-points at a foot distance; without vision. Nothing, I think,

[1] *Annales de Chimie et de Physique*, tom. lx. p. 71.

could more forcibly illustrate the special relationship which subsists between the optic nerve and the oscillating periods of the molecules of luminous bodies. That nerve, like a musical string, responds to the waves with which it is in accordance, while it refuses to be excited by others of almost infinitely greater energy which are not in unison with its own.

When we see a vivid light incompetent to affect our most delicate thermoscopic apparatus, the idea naturally presents itself that light and heat must be totally different things. The pure light emerging from a combination of water and green glass, even when rendered intense by concentration, has, according to Melloni, no sensible heating power. The light of the moon is also a case in point Concentrated by a polyzonal lens more than a yard in diameter upon the face of his pile, it required all Melloni's acuteness to *nurse* the calorific action of the moon up to a measurable quantity. Such experiments, however, demonstrate, not that the two agents are dissimilar, but that the sense of vision can be excited by an amount of energy almost infinitely small.[1]

Here also we are able to offer a remark as to the applicability of radiant heat to fog-signalling. The proposition, in the abstract, is a philosophical one; for were our fogs of a physical character, similar to that of the iodine held in solution by the bisulphide of carbon, or to that of iodine or bromine vapour, it would be possible to transmit through them, from our signal lamps, powerful fluxes of radiant heat, even after the entire stoppage of the light. But our fogs are not of this character. They are unfortunately so constituted as to act very destructively upon the purely calorific rays; and this fact, taken in con-

[1] With more powerful apparatus Lord Rosse, as already stated, has exhaustively examined the thermal lunar radiation, in all the phases of the satellite.

junction with the marvellous sensitiveness of the eye, leads to the conclusion, that long before the light of our signals ceases to be visible, their radiant heat has lost the power of affecting, in any sensible degree, the most delicate thermoscopic apparatus that we could apply to their detection.

LECTURE XVI.

ACTION OF ETHER WAVES OF SHORT PERIOD UPON GASEOUS MATTER—CLOUDS FORMED BY ACTINIC DECOMPOSITION—COLOUR PRODUCED BY SMALL PARTICLES—POLARISATION OF LIGHT BY NEBULOUS MATTER—CONSTITUTION OF THE SKY AND THE POLARISATION OF ITS LIGHT.

INTERACTION OF MOLECULES, ATOMS, AND ETHER WAVES.

IN the investigations on radiant heat thus placed in abstract before you, my chief aim was to render the longer waves of the prismatic spectrum interpreters and expositors of molecular condition. Unlike the beautiful researches of Melloni and Knoblauch, these investigations made radiant heat a means to an end. In describing them I have tried to place before your minds such images of molecules, and their constituent atoms, as modern science assumes, and such images of the luminiferous ether and its motions as the undulatory theory of light enables us to form, and to found upon these conceptions experimental inquiries which should give us a more sure and certain hold of the ultimate constitution of matter.

One result, among many now known to you, is the sudden change of relation between the ether and ordinary matter, which accompanies the act of chemical combination. Preserving the quantity, and ultimate quality, of the matter traversed by the ethereal waves constant, vast changes in the amount of wave-motion intercepted, may be produced by the act of chemical union. If nitrogen and oxygen, for example, be mixed mechanically together

in the proportion, by weight, of seven to four, radiant heat will pass through the mixture as through a vacuum. At all events, the quantity of heat intercepted is multiplied a thousandfold, the moment the oxygen and nitrogen combine to form laughing-gas. So, in like manner, if nitrogen and hydrogen be mixed mechanically in the proportion of fourteen to three, the amount of radiant heat absorbed by the mixture is multiplied by thousands—it may be by millions—the moment the gases unite chemically to form ammonia. No single experiment proves the air we breathe to be a mechanical mixture, and not a chemical compound, with the same conclusiveness, as that which establishes its deportment towards heat.

But the molecules which, like those of ammonia and laughing-gas, can intercept the waves of ether, must be shaken by those waves—possibly shaken asunder. That ordinary thermometric heat can produce chemical changes is one of the commonest facts. Radiant heat also, if sufficiently intense, and if absorbed with sufficient avidity, could produce all the effects of ordinary thermometric heat. The dark rays, for example, which can make platinum white-hot, could also, if absorbed, produce the chemical effects of white-hot platinum. They could, for example, decompose water, as they can now in a moment boil water. But the decomposition in this case would be effected through the virtual conversion of the radiant heat into thermometric heat. There would be nothing in the act characteristic of radiation, or demanding it as an essential element in the decomposition.

The chemical actions for which the radiant form seems essential are frequently produced by the least energetic rays of the spectrum. Thus the photographer has his heat-focus in advance of his chemical focus; which latter, though potent for his special purpose, possesses almost infinitely less mechanical energy than its neighbour. The

mechanical energy depends, as you know, upon the amplitude, or range of vibration; and as the heat waves have enormously greater amplitudes than the photographic waves, the decomposition, in this case, must be less a matter of amplitude than of period of vibration. The quicker motions of the shorter waves are so related to the vibrations possible to the atoms that, like the timed impulses of a boy in a swing, they accumulate so as finally to jerk the atoms asunder; thus effecting what is called chemical decomposition.

It is this jerking asunder of the constituent atoms of molecules by the shorter waves that we have to examine during the coming hour. The discovery of this action occurred thus:—Vapours of various kinds were sent into a glass tube a yard in length, and about three inches in diameter. As a general rule, the vapours were perfectly transparent; the tube, when they were present, appearing as empty as when they were absent. In two or three cases, however, a faint cloudiness showed itself within the tube. This caused me a momentary anxiety for I did now know how far, in describing my previous experiments, actions might have been ascribed to pure cloudless vapour, which were really due to those newly-observed nebulæ. Intermittent discomfort, however, is a necessary feeling of the investigator; for it drives him to closer scrutiny, to greater accuracy, and often, as a consequence, to new discovery. It was soon found that the nebulæ revealed by the beam were all *generated* by the beam; and the observation opened a new door into those regions of atoms and molecules, inaccessible to sense, but which embrace so much of the intellectual life of the physical investigator.

Those vapours of which we have been speaking are aggregates of *molecules*, and every molecule is, as already

explained, an aggregate of smaller parts, called *atoms*.[1] The molecules have motions of their own *as wholes*; their constituent atoms have also motions of their own, which are executed independently of those of the molecules; just as the various movements of bodies on the earth's surface are executed independently of the orbital revolution of our planet. The vapour molecules are kept asunder by forces which, virtually or actually, are forces of repulsion. Between these elastic forces and the atmospheric pressure under which the vapour exists, equilibrium is established, as soon as the proper distances between the molecules have been assumed. If, after this, the molecules be urged nearer to each other by an external force, they recoil as soon as the force is expended. If by the exercise of a similar force they be pushed more widely apart, when the force ceases to act they again approach each other. The case is different as regards the constituent atoms.

And here let me remark that we are now upon the outmost verge of molecular physics; and that I am attempting to familiarise your minds with conceptions which have not yet obtained universal currency even amongst chemists; which some chemists, moreover, might deem untenable. But, tenable or untenable, it is of the highest scientific importance to discuss them. Let us, then, look mentally at our atoms grouped together to form a molecule. Every atom is held apart from its neighbours by a force of repulsion; why, then, do not the mutually repellent members of this group part company? The molecules *do* separate when the external pressure is lessened or removed, but their constituent atoms do not. The reason

[1] Newton seemed to consider that the molecules might be rendered visible by microscopes; but of atoms he appears to have entertained a different opinion. 'It seems impossible,' he says, 'to see the more secret and noble works of nature within the corpuscles, by reason of their transparency.' (Herschel, 'On Light,' art. 1145.)

of this stability is that *two* forces, the one attractive and the other repulsive, are in operation between every two atoms; and the position of every atom is determined by the equilibration of these two forces. When the atoms approach too near each other, repulsion predominates and drives them apart; when they recede to too great a distance, attraction predominates and draws them together. The point at which attraction and repulsion are equal to each other is the atom's position of equilibrium. If not absolutely cold—and there is no such thing as absolute coldness in our corner of nature—the atoms are always in a state of vibration, their vibrations being executed to and fro across their positions of equilibrium.

Into a vapour thus constituted, we have now to pour a strong beam of white light. You know that the waves of this beam are not all of the same size; that some of them are much longer than others; that the short waves and the long ones move with the same rapidity through space, just as short and long waves of sound travel with the same rapidity through air, and that hence the shorter waves must follow each other in quicker succession than the longer ones. The elements of all the conceptions with which we shall have to deal are now in your possession. And you will observe that though we are speaking of things which lie entirely beyond the range of the senses, the conceptions are as truly mechanical as they would be if we were dealing with ordinary masses of matter, and with waves of sensible size.

Whether we see rightly or wrongly—whether our notions be real or imaginary—it is of the utmost importance in science to aim at perfect clearness in the description of all that comes, or seems to come, within the range of the intellect. For if we are right, clearness of utterance forwards the cause of right; while if we are wrong, it ensures the speedy correction of error. In this spirit,

and with the determination at all events to speak plainly, let us deal with our conceptions of ether waves and molecules. Supposing a wave, or a train of waves, to impinge upon a molecule so as to urge all its parts with the same motion, the molecule would move bodily as a whole, but, because they are animated by a common motion, there would be no tendency on the part of its constituent atoms to separate from each other. *Differential motions* among the atoms themselves would be necessary to effect a separation; and if such motions be not introduced by the shock of the waves, there is no mechanical ground for the decomposition of the molecule.

It is, however, difficult to conceive the shock of a wave, or a train of waves, so distributed among the atoms as to cause no strain amongst them. For atoms are of different weights, probably of different sizes; at all events it is almost certain that the ratio of the mass of the atom, to the surface which it presents to the impact of the ether waves, is different in different cases. If this be so, and I think the probabilities are immensely in favour of its being so, then every wave which passes over a molecule tends to decompose it—tends to carry away from their weightier and more sluggish companions those atoms which, in relation to their mass, present the largest resisting surfaces to the motion of the waves. The case may be illustrated by reference to a man standing on the deck of a ship. As long as both of them share equally the motions of the wind, or of the sea, there is no tendency to separation. In chemical language, they are in a state of combination. But a wave passing over it finds the ship less rapid in yielding to its motion than the man; the man is consequently carried away, and we have what may be roughly regarded as decomposition.

Thus the conception of the decomposition of compound molecules by the waves of ether comes to us recommended

by *à priori* probability. But a closer examination of the question compels us to supplement, if not materially to qualify, this conception. It is a most remarkable fact, that the waves which are most effectual in shaking asunder the atoms of compound molecules are frequently those of least mechanical power. Billows, to use a strong comparison, are incompetent to produce effects which are readily produced by ripples. The violet and ultra-violet rays of the sun, for example, are often most effectual in producing chemical decomposition; and compared with the red and ultra-red solar rays, the energy of the 'chemical rays' is infinitesimal. This energy would probably, in some cases, have to be multiplied by millions, to bring it up to that of the ultra-red rays. Still the latter are often powerless where the smaller waves are potent. We here observe a remarkable similarity between the behaviour of chemical molecules and that of the human retina, for the capacity to produce light does not depend on the energy of the waves, the most powerful solar waves failing entirely to excite vision.

Whence, then, the power of these smaller waves to unlock the bonds of chemical union? If it be not a result of their own strength, it must be, as in the case of vision, a result of their periods of recurrence. But how are we to figure this action? I should say thus: the shock of a single wave produces no more than an infinitesimal effect upon an atom or a molecule. To produce a larger effect, the motion must accumulate, and for wave-impulses to accumulate, they must arrive in periods identical with the periods of vibration of the atoms on which they impinge. In this case each successive wave finds the atom in a position which enables that wave to add its shock to the sum of the shocks of its predecessors. The single tick of a clock has no appreciable effect upon the unvibrating and equally long pendulum of a distant clock; but a

succession of ticks each of which adds, at the proper moment, its infinitesimal push to the sum of the pushes preceding it, will, as a matter of fact, set the second clock going. So likewise a single puff of air against the prong of a heavy tuning-fork produces no sensible motion, and, consequently, no audible sound; but a succession of puffs, which follow each other in periods identical with the tuning-fork's period of vibration, will render the fork sonorous. I think the chemical action of light is to be regarded in this way. Fact and reason point to the conclusion that it is the heaping up of motion on the atoms, in consequence of their synchronism with the shorter waves, that causes them to part company.

ACTINIC CLOUDS.

And now let us return to that faint cloudiness, already mentioned, from which, as from a germ, these considerations and speculations have sprung. It has been long known that light effected the decomposition of a certain number of bodies. The transparent iodide of ethyl, or of methyl, for example, becomes brown and opaque on exposure to light, through the discharge of its iodine. The art of photography is founded on the chemical actions of light; so that it is well known that the effects for which the foregoing theoretic considerations would have prepared us, are not only probable, but actual.

But the method now to be followed, and which consists simply in offering the *vapours* of volatile substances to the action of light, enables us to give a vast extension to the operations of light, or rather of radiant force, as a chemical agent. It also enables us to imitate in our laboratories actions which have been hitherto performed only in the laboratory of nature. The substances chosen for examination are so constituted that when their molecules are

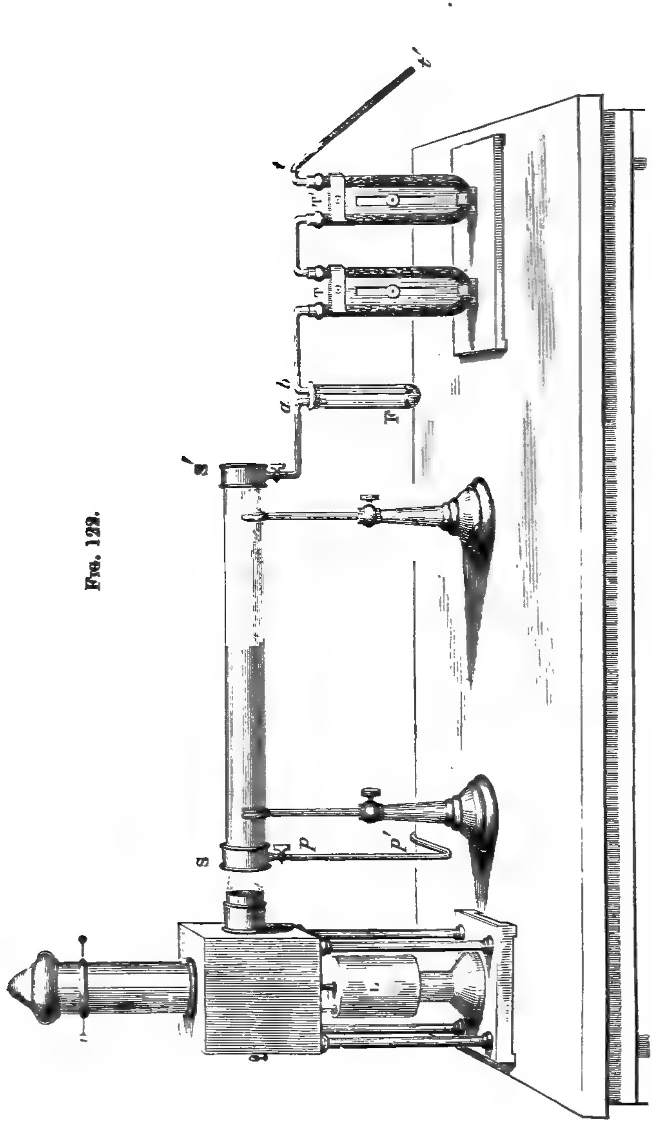


Fig. 129.

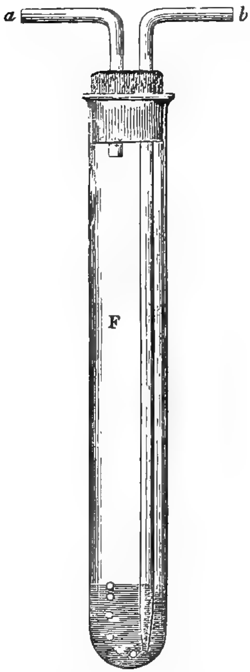
broken up by the waves of light, the newly-formed bodies are comparatively *involatile.* To keep them in the gaseous form these products of decomposition require a higher temperature than the vapours from which they are derived; hence, if the space in which these new bodies are liberated be of a lower temperature than that requisite to maintain the vaporous condition, they will be precipitated, as clouds, upon the beam, to the action of which they owe their existence.

The simple apparatus employed in these experiments will be at once understood by reference to fig. 122. S S′ is a glass experimental tube, which may vary in length from one to five feet, with a diameter of two or three inches. From the end S the pipe *p p′* passes to an air-pump. Connected with the other end is the flask F, containing the liquid whose vapour is to be examined Then follows a U-tube T, filled with fragments of clean glass wetted with sulphuric acid. The air is here dried. Then follows a second U-tube T′ containing fragments of marble wetted with caustic potash. The carbonic acid of the air is here removed. Finally comes a narrow tube *t t′*, containing a tolerably tightly-fitting plug of cotton-wool. This intercepts the floating matter of the air. To save the air-pump gauge from the attack of such vapours as act upon mercury, as also to facilitate observation, a separate barometer-tube is employed.

The flask F, fig. 122, is shown on an enlarged scale in fig. 123. Through its cork two glass tubes *a* and *b* pass air-tight. The tube *a* ends immediately under the cork; the tube *b*, on the contrary, descends to the bottom of the flask, and dips into the liquid. The end of this tube is drawn out so as to render very small the orifice through which the air escapes into the liquid. The experimental tube S S′ being exhausted, a cock at the end S′ is carefully turned on. The air passes slowly through the

cotton-wool, the caustic potash, and the sulphuric acid, in succession. Thus purified, it enters the flask F, and bubbles through the liquid Charged with vapour it finally enters the experimental tube, where it is subjected to examination. The lamp L, placed at the end of the experimental tube, furnishes the necessary beam.

FIG. 123



We will now permit the electric beam to play upon the invisible vapour of nitrite of amyl. The lens of the lamp is so situated as to render the beam convergent, the focus falling near the middle of the tube. You will notice that the tube appears empty for a moment after the turning on of the beam; but the chemical action will be so rapid that attention is requisite to mark this interval of darkness. I ignite the lamp, and a luminous white cloud immediately falls upon the beam. The beam has, in fact, shaken asunder the molecules of the nitrite of amyl, and brought down upon itself a shower of particles which flash forth like a solid luminous spear. This experiment, moreover, illustrates the fact, that however intense a beam of light may be, it remains invisible unless it has something to shine upon. *Space*, though traversed by the rays from all suns and all stars, is itself unseen. Not even the ether which fills space, and whose motions are the light of the universe, is itself visible.

You notice that the end of the experimental tube

most distant from the lamp is free from cloud. Now the nitrite of amyl vapour is there also, but it is unaffected by the powerful beam passing through it. Let us reconcentrate the transmitted beam by receiving it on a concave silvered mirror, and cause it to return into the tube. It is still powerless. Though a cone of light of extraordinary intensity now traverses the vapour, no precipitation occurs, and no trace of cloud is formed. Why? Because only a very small fraction of the beam possesses the power of decomposing the vapour; and this fraction is quite exhausted by its work in the frontal portion of the tube. The great body of the light which remains, after this sifting out of the few effectual rays, has no power over the molecules of nitrite of amyl. We have here strikingly illustrated, what has been already stated regarding the influence of *period*, as contrasted with that of *strength*. For the portion of the beam which is here ineffectual has probably more than a million times the absolute energy of the effectual portion. It is energy specially related to the atoms that we here need, which specially related energy, being possessed by the feeble waves, invests them with their extraordinary power. When the experimental tube is reversed so as to bring the undecomposed vapour under the action of the *unsifted* beam, we have instantly a fine luminous cloud precipitated.

The light of the sun also effects the decomposition of the nitrite of amyl vapour. To prove this a sunbeam is converged so as to form a luminous cone, visible in the dust of the room. On thrusting one end of the vapour-filled tube into the light behind the lens, precipitation within the cone is copious and immediate. As before the vapour at the distant end of the tube is shielded by that in front; but on reversing the tube, a second and similar cloud-cone is precipitated.

And here I would ask you to make familiar to your

minds the idea that no chemical action can be produced by a ray that does not involve the destruction of the ray. But abandoning the term ray as loose and indefinite, let us fix our thoughts upon the *waves* of light, and render clear to our minds that those waves which produce chemical action do so by delivering up their own motion to the molecules which they decompose. We are here in the presence of a question of great importance in molecular physics; it is this: When the waves of ether are intercepted by a compound vapour, is the motion of the waves transferred to the molecules of the vapour, or to the atoms of the molecules? We have thus far leaned to the conclusion that the motion is communicated to the atoms; for if not to these individually, why should they be shaken asunder? The question, however, is capable of, and is worthy of, another test, the bearing and significance of which you will immediately appreciate.

As already explained, vapour molecules are held in their positions by their mutual repulsion on the one side, and by an external pressure on the other. Like a stretched string, their rate of vibration, if they vibrate at all, must depend upon the elastic force existing between them. If this force were changed, the rate of vibration would change along with it; and, after the change, the molecules could no longer absorb the waves which they absorbed prior to the change. Now the elastic force between molecule and molecule is utterly altered when a vapour passes to the liquid state. Hence if the liquid absorb waves of the same period as its vapour, it is a proof that the absorption is not the act of the molecules. Let us be perfectly clear on this important point. Waves are absorbed whose vibrations synchronise with those of the molecules or atoms on which they impinge. If then, after the passage of vapour to the liquid state, the same waves be absorbed as were absorbed prior to the passage, it is a

proof that the molecules which have utterly changed *their* periods, cannot be the seat of the absorption; and we are driven to conclude that it is to the *atoms*, whose rates of vibration are unchanged by the change of aggregation, that the wave motion is transferred. If experiment should prove this identity of action on the part of a vapour and its liquid, it would establish in a new and striking manner the conclusion to which we have previously leaned.

We will now resort to the experimental test. In front of the experimental tube, which contains a quantity of the nitrite of amyl vapour, is placed a glass cell a quarter of an inch in thickness, filled with the liquid nitrite of amyl. I send the electric beam first through the liquid and then through its vapour. The luminous power of the beam is very great, but it can make no impression upon the vapour. The liquid has robbed it completely of its effective waves. On the removal of the liquid, chemical action immediately begins, and in a moment we have the apparently empty tube filled with a bright cloud, precipitated by one portion of the beam, and illuminated by another. I reintroduce the liquid: the chemical action instantly ceases. I again remove it, and the action commences once more. Thus we uncover, in part, the secrets of this world of molecules and atoms.

Instead of employing air as the vehicle by which the vapour is carried into the experimental tube, we may employ oxygen, hydrogen, or nitrogen; and besides the nitrite of amyl, a great number of other substances might be employed, which, like the nitrite, have been hitherto not known to be chemically susceptible to light. One point in addition I wish to illustrate, chiefly because the effect is similar in kind to one of great importance in nature. In our atmosphere floats carbonic acid, which furnishes food to the vegetable world. But this food

could not be consumed by plants and vegetables without the intervention of the sun's rays. And yet, as far as we know, these rays are powerless upon the free carbonic acid of our atmosphere. The sun can only decompose the gas when it is absorbed by the leaves of plants. In the leaves it is in close proximity with substances ready to take advantage of the loosening of its molecules by the waves of light. Incipient disunion being thus introduced, the carbon of the gas is seized upon by the leaf and appropriated, while the oxygen is discharged into the atmosphere.

The experimental tube now before you contains a different vapour from that which we have hitherto employed. It is called the nitrite of butyl. On sending the electric beam through the tube the chemical action is scarcely sensible. I add to the vapour a quantity of air which has been permitted to bubble through hydrochloric acid. When the beam is afterwards turned on, so rapid is the action, and so dense the cloud precipitated, that you could hardly by an effort of attention observe the interval which preceded the precipitation. This enormous augmentation of the action is due to the presence of the hydrochloric acid. Like the chlorophyl and carbonic acid in the leaves of plants, the two substances interact under the influence of the waves of the electric light.

The nitrite of amyl furnishes a similar example. The decomposition of this substance by light is very energetic when alone, but the energy and brilliancy of the action are greatly augmented by the presence of hydrochloric acid. Air which has bubbled through the liquid nitrite is admitted into this experimental tube till the mercury gauge of the pump has sunk eight inches. Eight additional inches of air which has bubbled through liquid hydrochloric acid are then admitted. On permitting the powerful beam of the electric lamp to act upon the mix-

ture, a cloud of extraordinary density and brilliancy is immediately precipitated on the beam, which seems to pierce like a share the shining nebula, tossing in heaps the precipitated particles right and left as it advances among them.

A minute but interesting variation of the experiment is this: the nozzle of a bellows being connected by a bit of india-rubber tubing with a glass tube passing through a cork into a vessel containing the nitrite vapour, a sharp tap on the bellows sends a puff of the vapour through a second open tube passing through the same cork. In ordinary diffuse light the puff of vapour is invisible. Projected into a concentrated sunbeam, or into the beam from the electric lamp, on crossing the limit of light and darkness the vapour is instantly precipitated as cloud, and forms a shining white ring. This ring has the same mechanical cause as the smoke rings puffed from the mouth of a cannon; but it is latent until revealed by actinic precipitation.

THE AZURE OF THE FIRMAMENT.

It is possible to impart to these clouds any required degree of tenuity, for it is in our power to limit at pleasure the amount of vapour in our experimental tube. When the quantity is duly limited, the precipitated particles are at first inconceivably small, defying the highest microsopic power to bring them within range of the vision. Probably their diameters are then not greater than the millionth of an inch. They grow gradually, and as they augment in size, they throw from them a continually increasing quantity of wave-motion, until, finally, the cloud which they form becomes so luminous as to fill a room with light. During the growth of the particles the most splendid iridescences are often exhibited. It is not, however, with

the iridescences, however beautiful they may be, that we have now to occupy our thoughts, but with other effects which bear upon the two great standing enigmas of meteorology—the colour of the sky and the polarisation of its light.

First, then, with regard to the sky; how is it produced, and can we not reproduce it? Its colour has not the same origin as that of ordinary colouring matter, in which certain portions of the white solar light are absorbed, the colour of the body being that of the light which remains. A violet is blue because its molecular texture enables it to quench the yellow and red constituents of white light, and to send back the blue from its interior. A geranium is red because its molecular texture is such as quenches all rays except the red. Such colours are called colours of absorption; but the hue of the sky is not of this character. The blue light of the sky is scattered light; and, were there nothing in our atmosphere competent to scatter the solar rays, we should see no blue firmament, but the mere darkness of infinite space. The blue of the sky is produced by perfectly colourless particles. Smallness of size alone is requisite to ensure the selection and reflection of this colour. Of all the visual waves emitted by the sun, the shortest and smallest are those corresponding to the colour blue. To such small waves minute particles offer more obstruction than to large ones, hence the predominance of blue colour in all light reflected from such particles. The crimson glow of the evening and the morning, seen so finely in the Alps, is due, on the other hand, to *transmitted* light; that is to say, to light which, in its passage through great atmospheric distances, has its blue constituents sifted out of it by repeated collision with suspended particles.

It is possible, as above stated, by duly regulating the quantity of vapour, to make our precipitated particles

grow from an infinitesimal, and altogether ultra-microscopic size, to specks of sensible magnitude; and by means of these particles, in a certain stage of their growth, we can produce a blue which shall rival, if it does not transcend, that of the deepest and purest Italian sky. Let this point be in the first place established. Associated with our experimental tube is a barometer, the mercurial column of which now indicates that the tube is exhausted. Into the tube I introduce a quantity of the mixed air and nitrite of butyl vapour, sufficient to depress the mercurial column one-twentieth of an inch; that is to say, the air and vapour together exert a pressure of one six-hundredth of an atmosphere. I now add a quantity of air and hydrochloric acid, sufficient to depress the mercury half-an-inch further, and into this compound and highly attenuated atmosphere I discharge the beam of the electric light. The effect is slow; but gradually within the tube arises a splendid azure, which strengthens for a time, reaches a maximum of depth and purity, and then, as the particles grow larger, passes into whitish blue. This experiment is representative, and it illustrates a general principle. Other colourless substances of the most diverse properties, optical and chemical, might be employed for this experiment. The *incipient cloud*, in every case, would exhibit this superb blue; thus proving to demonstration, that particles of infinitesimal size, without any colour of their own, and irrespective of the optical properties exhibited by the substances in a massive state, are competent to produce the colour of the sky.

POLARISATION OF SKY-LIGHT.

But there is still another subject connected with our firmament, of a more subtle and recondite character than even its colour. I mean that 'mysterious and beautiful

phenomenon,'[1] the polarisation of the light of the sky. Brewster, Arago, Babinet, Herschel, Wheatstone, Rubeson and others, have made us masters of the phenomenon, but its cause remains a mystery still. The polarity of a magnet consists in its *two-endedness*, both ends, or poles, acting in opposite ways. Polar forces, as most of you know, are those in which the duality of attraction and repulsion is manifested. And a kind of *two-sidedness*—noticed by Huygens, commented on by Newton, and more fully observed and investigated by a French philosopher, named Malus, receives the name of *polarisation*. We must now, however, attach a distinctness to the idea of a polarised beam, which its discoverers were not able to affix to it. For in their day men's thoughts were not sufficiently ripe, nor optical theory sufficiently advanced, to seize upon or express the physical meaning of polarisation. We have already learned that in the case of sound, the vibrations of the air-particles are executed in the direction in which the sound travels. They are therefore called longitudinal vibrations. In the case of light and radiant heat, on the contrary, the vibrations are transversal; the individual particles of ether move to and fro *across* the direction in which the light is propagated. In the case of a common beam of light, the vibrations of the ether particles are executed in every direction perpendicular to it; but if the beam impinge obliquely, upon a plane glass surface, as in the observation of Malus, the portion reflected will no longer have its particles vibrating in all directions round it. By the act of reflection, *if it occur at the proper angle*, the vibrations are all confined to a single plane, and light thus circumstanced is called *plane polarised light.*

A beam of light passing through ordinary glass ex-

[1] Herschel's 'Meteorology,' art. 233.

ecutes its vibrations within the substance exactly as it would do in air, or in ether-filled space. Not so when it passes through many transparent crystals. For these also have their two-sidedness, the arrangement of their molecules being such as to tolerate vibrations only in certain definite directions. There is the well-known crystal tourmaline, which shows a marked hostility to all vibrations executed at right angles to the axis of the crystal. It speedily extinguishes such vibrations, while those executed parallel to the axis are more or less freely propagated. The consequence is, that a beam of light, after it has passed through any thickness of this crystal, emerges from it polarised. So, also, as regards the beautiful crystal known as Iceland spar, or as double-refracting spar. In one direction, but in one only, this crystal shows the neutrality of glass; in all other directions it splits the beam of light passing through it into two distinct halves, both of which are perfectly polarised, their vibrations being executed in two planes, at right angles to each other.

It is possible by a suitable contrivance to get rid of one of the two polarised beams, into which Iceland spar divides an ordinary beam of light. This was done so ingeniously and effectively by Nicol, that the spar, cut in his fashion, is now universally known as Nicol's prism. Such a prism can polarise a beam of light, and if the beam, before it impinges on the prism, be already polarised, in one position of the prism it is stopped, while in another position it is transmitted. The same is true of radiant heat.[1] Our way is now, to some extent, cleared towards the examination of the light of the sky.

Looking at various points of the blue firmament through a Nicol's prism, and turning the prism round its axis, we soon notice variations of the brightness of the sky.

[1] The total reflection, polarisation, and magnetisation of Radiant Heat are fully illustrated in my Lectures on Light, 2nd ed. p. 181 *et seq.*

In certain positions of the spar, and from certain points of the firmament, the light appears to be freely transmitted; while it is only necessary to turn the prism round its axis through an angle of ninety degrees to materially diminish the intensity of the light. On close scrutiny it is found, that the difference produced by the rotation of the prism is greatest, when the sky is regarded in a direction at right angles to that of the solar rays. Experiments of this kind prove that the blue light sent to us by the firmament is polarised, and that the direction of most perfect polarisation is perpendicular to the solar rays. Were the heavenly azure like the ordinary light of the sun, the turning of the prism would have no effect upon it; it would be transmitted equally during the entire rotation of the prism. The light of the sky is in great part quenched by the Nicol, because it is in great part polarised.

When a luminous beam impinges at the proper angle on a plane glass surface it is polarised by reflection. It is polarised, in part, by all oblique reflections; but at one particular angle, the reflected light is perfectly polarised. An exceedingly beautiful and simple law, discovered by Sir David Brewster, enables us readily to find the polarising angle of any substance whose refractive index is known. This law was discovered experimentally by Brewster; but the Wave Theory of light renders a complete reason for the law. A geometrical image of it is thus given. When a beam of light impinges obliquely upon a plate of glass it is in part reflected and in part refracted. At one particular incidence the reflected and the refracted portions of the beam are at right angles to each other. The angle of incidence is then the polarising angle. It varies with the refractive index of the substance; being for water $52\frac{1}{2}$, for glass $57\frac{1}{2}$, and for diamond 68 degrees.

And now we are prepared to comprehend the diffi-

culties which have beset the question before us. It has been already stated that in order to obtain the most perfect polarisation of the firmamental light, the sky must be regarded in a direction at right angles to the solar beams. This is sometimes expressed by saying that the place of maximum polarisation is at an angular distance of 90° from the sun. This angle, enclosed as it is between the direct rays and those sent from the sky, comprises both the angles of incidence and reflection. Hence the angle of incidence, which corresponds to the maximum polarisation of the sky, is half of 90°, or 45°. This is the atmospheric polarising angle, and the question is, what known substance possesses an index of refraction to correspond with this polarising angle? If we knew such a substance, we might be tempted to conclude that particles of it, scattered in the atmosphere, produce the polarisation of the sky. 'Were the angle of maximum polarisation,' says Sir John Herschel, '76° (instead of 90°), we should look to *water*, or ice, as the reflecting body, however inconceivable the existence in a cloudless atmosphere, and a hot summer day, of unevaporated particles of water.' But a polarising angle of 45° corresponds to a refractive index of 1; this means that there is no refraction at all, in which case we ought to have no reflection. To satisfy the law of Brewster, as Sir John Herschel remarks, 'the reflection would have to be made *in* air *upon* air!' 'The more the subject is considered,' adds the celebrated philosopher last named, 'the more it will be found beset with difficulties, and its explanation, when arrived at, will probably be found to carry with it that of the blue colour of the sky itself.'

ARTIFICIAL SKY.

If you doubt the wisdom, acknowledge, at all events, the faith in your capacity, which has caused me to bring a

subject so entangled before you. I believe, however, that even an intellect which draws its chief strength and culture from totally different sources, may have its interest excited by subjects like the present, dark and difficult though they be. It is not to be expected that you will all grasp the details of this discussion; but I think that everybody present will see the extremely important part hitherto played by the law of Brewster in speculations as to the colour and polarisation of the sky. I shall now seek to demonstrate in your presence, first, and in confirmation of our former experiments, that sky-blue may be produced by exceedingly minute particles of any kind; secondly, that polarisation identical with that of the sky is produced by such particles; and thirdly, that matter in this fine state of division, where its particles are probably small in comparison with the height and span of a wave of light, releases itself completely from the law of Brewster; the direction of maximum polarisation being absolutely independent of the polarising angle as hitherto defined.

Into an experimental tube I introduce a new vapour in the manner already described, and add to it air which has been permitted to bubble through dilute hydrochloric acid. On permitting the electric beam to play upon the mixture, for some time nothing is seen. The chemical action is doubtless progressing, and condensation is going on; but the condensing molecules have not yet coalesced to particles sufficiently large to scatter sensibly the waves of light. As before stated—and the statement rests upon an experimental basis—the particles here generated are at first so small, that their diameters do not probably exceed a millionth of an inch: while to form each of these particles whole crowds of molecules are probably aggregated. Helped by such considerations, our intellectual vision plunges more profoundly into atomic nature, and shows

us, among other things, how far we are from the realisation of Newton's hope that the molecules might one day be seen by means of microscopes. While I am speaking, you observe this delicate blue colour forming and strengthening within the experimental tube. No sky-blue could exceed it in richness and purity; but the particles which produce this colour lie wholly beyond our microscopic range. A uniform colour is here developed, which has as little breach of continuity—which yields as little evidence of the individual particles concerned in its production—as that yielded by a body whose colour is due to true molecular absorption. This blue is at first as deep and dark as the sky seen from the highest Alpine peaks, and for the same reason. But it grows gradually brighter, still maintaining its blueness, until at length a whitish tinge mingles with the pure azure; announcing that the particles are now no longer of that infinitesimal size which scatters only the shortest waves.[1]

The liquid here employed is the iodide of allyl,[2] but I might choose any one of a dozen substances here before me to produce the effect. You have seen what may be done with the nitrite of butyl. With nitrite of amyl, bisulphide of carbon, benzol, benzoic ether, &c. the same blue colour may be produced.[3] In all cases where matter slowly passes from the molecular to the massive state, the transition is marked by the production of the blue. More than this:—you have seen me looking at the blue colour (I hardly like to call it a blue 'cloud,' its texture and properties are so different from ordinary clouds) through a bit

[1] Possibly a photographic impression might be taken before the blue becomes visible, for the ultra-blue rays are first scattered.

[2] For which I have to thank the obliging kindness of Dr. Maxwell Simpson.

[3] To the list of gaseous bodies decomposed by light and producing actinic clouds, M. Morren has added sulphurous acid, and Professor Dewar the very sensitive peroxide of chlorine.

of spar. This is a Nicol's prism. The blue that I have been thus looking at is a bit of more perfect sky than the sky itself. Looking across the illuminating beam as we look across the solar rays at the sky, I obtain not only partial polarisation, but *perfect* polarisation. In one position of the Nicol the blue light seems to pass unimpeded to the eye; in the other it is absolutely cut off, the experimental tube being reduced to optical emptiness. Behind the experimental tube it is well to place a black surface, in order to prevent foreign light from troubling the eye. In one position of the Nicol this black surface is seen without softening or qualification; for the particles within the tube are themselves invisible, and the light which they scatter is quenched. If the light of the sky were polarised with the same perfection, on looking properly towards it through a Nicol we should also meet, not the mild radiance of the firmament, but the unillumined blackness of space.

The construction of the Nicol is such that it permits free passage to vibrations which are executed in a certain determinate direction. All vibrations executed at right angles to this direction are completely stopped; while components, only, of those executed obliquely to it are transmitted. It is easy, therefore, to see that from the position in which the Nicol must be held, to transmit or to quench the light of our actinic cloud, we can infer the direction of the vibrations of that light. You will be able to picture those vibrations without difficulty. Suppose a line drawn from any point of the 'cloud' perpendicular to the illuminating beam. The particles of ether which carry the light along that line, from the cloud to the eye, vibrate in a direction perpendicular both to the line and to the beam. And if any number of lines be drawn in the same way from the cloud, like the spokes of a wheel, the particles of ether along all of them os-

cillate in the same manner. Wherefore, if a *plane surface* be imagined cutting the incipient cloud at right angles to its length, the perfectly polarised vibrations discharged laterally will all be parallel to this surface. This is the plane of vibration of the polarised light. Or you may suppose a circle drawn round the experimental tube on its surface, and a series of strings attached to various points of this circle. If all the strings be stretched as perpendiculars to the experimental tube, and caused to vibrate transversely by a series of jerks imparted at right angles both to them and the tube, the motion of the particles of the strings will then represent those of the particles of ether. A distinct image of those vibrations is now, I hope, in your minds.

Our actinic cloud is a virtual Nicol's prism, and between it and the real Nicol, we can produce all the effects obtainable between the polariser and analyser of a polariscope. When, for example, a thin plate of selenite is placed between the Nicol and the incipient cloud, we obtain the splendid chromatic phenomena of polarised light. The colour of the gypsum-plate, as many of you know, depends upon its thickness. If this be uniform, the colour is uniform. If, on the contrary, the plate be wedge-shaped, thickening gradually and uniformly from edge to back, we obtain brilliant bands of colour parallel to the edge of the wedge. Perhaps the best form of the selenite for experiments of this character is a plate thin at the centre, and gradually thickening towards the circumference. Placing the film between the Nicol and the cloud, we obtain, instead of a series of parallel bands, a system of coloured rings. The colours are most vivid when the incipient cloud is looked at perpendicularly to the direction of the illuminating beam. Precisely the same phenomena are observed when we look at the blue firmament, in a direction perpendicular to the solar rays.

We have thus far operated with ordinary light, and found the portion of this light scattered normally to be perfectly polarised. We will now examine the effects produced when the light which illuminates the actinic cloud is itself polarised. In front of the electric lamp, and between it and the experimental tube, is placed a fine Nicol's prism, which is sufficiently large to embrace and to polarise the entire beam. The prism is now placed so that the plane of vibration of the light emergent from it, and falling upon the cloud, is vertical. How does the cloud behave towards this light? This formless aggregate of infinitesimal particles without definite structure, shows the two-sidedness of the light in the most striking manner. It is absolutely incompetent to send the light upwards or downwards, while it freely discharges the light horizontally, right and left. I turn the polarising Nicol so as to render the plane of vibration horizontal; the cloud now freely sends the light vertically upwards and downwards, but it is absolutely incompetent to shed a ray horizontally to the right or left. While showing him some of the foregoing effects this form of experiment was suggested to me by Professor Stokes.

Suppose the atmosphere of our planet to be surrounded by an envelope impervious to light, with an aperture on the sunward side, through which a solar beam could enter. Surrounded on all sides by air not directly illuminated, the track of the sunlight would resemble that of the electric beam in a dark space filled with our incipient cloud. The course of the sunbeam would be *blue*, and it would discharge laterally, in all directions round it, light in precisely the same polarised condition as that discharged from the incipient cloud. In fact, the azure revealed by the sunbeam would be the azure of such a cloud. And if, instead of permitting the ordinary light of the sun to enter the aperture, a Nicol's prism were placed

there, which should polarise the sunlight on its entrance into our atmosphere, the particles producing the colour of the sky would act precisely like those of our incipient cloud. In two directions we should have the solar light reflected; in two others unreflected. In fact, out of such a solitary beam, traversing the unilluminated air, we should be able to extract every effect shown by our incipient cloud. In the production of such clouds we virtually create bits of sky in our laboratories, and obtain with them all the effects obtainable in the open firmament of heaven. These experiments, and others that might be cited, render it certain that the blue of our firmament is that of light scattered by mechanically suspended particles.

NOTE.—Though not strictly belonging to heat, the subject of atomic motion is so strikingly illustrated by the researches referred to in the foregoing lecture, that I thought it desirable to introduce some account of them here.

LECTURE XVII.

DEW:—A CLEAR SKY AND CALM BUT DAMP ATMOSPHERE NECESSARY FOR ITS COPIOUS FORMATION—DEWED SUBSTANCES COLDER THAN UNDEWED ONES—DEWED SUBSTANCES BETTER RADIATORS THAN UNDEWED ONES—DEW IS PRODUCED BY THE CONDENSATION OF ATMOSPHERIC VAPOUR ON SUBSTANCES WHICH HAVE BEEN CHILLED BY RADIATION—LUNAR RADIATION—CONSTITUTION OF THE SUN—THE BRIGHT LINES IN THE SPECTRA OF THE METALS—AN INCANDESCENT VAPOUR ABSORBS THE RAYS WHICH IT CAN ITSELF EMIT—KIRCHHOFF'S GENERALISATION—FRAUNHOFER'S LINES—SOLAR CHEMISTRY—EMISSION OF THE SUN—HERSCHEL AND POUILLET'S EXPERIMENTS—MAYER'S METEORIC THEORIES—THEORIES OF WATERSTON, THOMSON, AND HELMHOLTZ—ENERGIES OF THE SOLAR SYSTEM—RELATION OF THE SUN TO ANIMAL AND VEGETABLE LIFE.

TERRESTRIAL RADIATION.

WE have learned that our atmosphere is always more or less charged with aqueous vapour, the condensation of which forms clouds, fogs, hail, rain, and snow. We have now to direct our attention to one particular case of condensation, of great interest and beauty—one, moreover, regarding which erroneous notions were for a long time entertained—the phenomenon of Dew. The aqueous vapour of our atmosphere is a powerful radiant, but it is diffused through air which usually exceeds its own mass one hundred times. Not only, then, its own heat, but the heat of the large quantity of air which surrounds it, must be discharged by the vapour, before it can sink to its point of condensation. The retardation of chilling, due to this cause, enables good solid radiators, at the earth's surface, to outstrip the vapour in speed of refrigeration; and

hence upon these bodies aqueous vapour may be condensed to liquid, or even congealed to hoar-frost, while at a few feet above the surface it maintains its gaseous state. This is actually the case in the beautiful phenomenon which we have now to examine.

We are indebted to a London physician for a true theory of dew. In 1818 Dr. Wells published his admirable essay on this subject. He made his experiments in a garden in Surrey, at a distance of three miles from Blackfriars Bridge. To collect the dew, he used little bundles of wool, which, when dry, weighed 10 grains each; and having exposed them during a clear night, the amount of dew deposited on them was determined by the augmentation of their weight. He soon found that whatever interfered with the view of the sky from his piece of wool, interfered also with the deposition of dew. He supported a board on four props: *on* the board he laid one of his wool parcels, and *under* it a second similar one; during a clear calm night, the former gained 14 grains in weight, while the latter gained only 4. He bent a sheet of pasteboard like the roof of a house, and placed underneath it a bundle of wool on the grass: by a single night's exposure the wool gained 2 grains in weight, while a similar piece of wool exposed on the grass, but unshaded by the roof, collected 16 grains of moisture.

Is it steam from the earth, or is it fine rain from the heavens, that produces this deposition of dew? Both of these notions have been advocated. That it does not arise from the earth is, however, proved by the fact, that more moisture was collected *on* the propped board than under it. That it is not a fine rain is proved by the fact that the most copious deposition occurs on the clearest nights.

Dr. Wells next exposed thermometers, as he had done his wool-bundles, and found that at those places where the

dew fell most copiously, the temperature sank lowest. On the propped board already referred to, he found the temperature 9° Fahr. lower than under it; beneath the pasteboard roof the thermometer was 10° warmer than on the open grass. He also found that when he laid his thermometer upon a grass plot, on a clear night, it sank sometimes 14° lower than a similar thermometer suspended in free air, at a height of 4 feet above the grass. A bit of cotton, placed beside the former, gained 20 grains; a similar bit, beside the latter, only 11 grains in weight. *The lowering of the temperature and the deposition of the dew went hand in hand.* Not only did artificial screens interfere with the lowering of the temperature and the formation of the dew, but a cloud-screen acted in the same manner. He once observed his thermometer, which, as it lay upon the grass, showed a temperature 12° Fahr. lower than the air a few feet above the grass, rise, on the passage of some clouds, until it was only 2° colder than the air. In fact, as the clouds crossed his zenith, or disappeared from it, the temperature of his thermometer rose and fell.

A series of such experiments, conceived and executed with admirable clearness and skill, enabled Dr. Wells to propound a Theory of Dew, which has stood the test of all subsequent criticism, and is now universally accepted.

It is an effect of chilling by radiation. 'The upper parts of the grass radiate their heat into regions of empty space, which, consequently, send no heat back in return; its lower parts, from the smallness of their conducting power, transmit little of the earth's heat to the upper parts, which, at the same time, receiving only a small quantity from the atmosphere, and none from any other lateral body, must remain colder than the air, and condense into dew its watery vapour, if this be sufficiently abundant in respect to the decreased temperature of the grass.' Why the vapour itself, being a powerful radiant,

is not so quickly chilled as the grass, has been already explained on the ground that the vapour has not only its own heat to discharge, but also that of the large mass of air by which it is surrounded.

Dew, then, is the result of the condensation of atmospheric vapour, on substances which have been sufficiently cooled by radiation; and as bodies differ widely in their radiative powers, we may expect corresponding differences in the deposition of dew. This Wells proved to be the case. He often saw dew copiously deposited on grass and painted wood, when none could be observed on gravel walks adjacent. He found plates of metal, which he had exposed, quite dry, while adjacent bodies were covered with dew. In all such cases the temperature of the metal was found to be higher than that of the dewed substances. This is quite in accordance with our knowledge that metals are the worst radiators. On one occasion he placed a plate of metal upon grass, and upon the plate a glass thermometer; the thermometer, after some time, exhibited dew, while the plate remained dry. This led him to suppose that the instrument, though lying on the plate, did not share its temperature. He placed a second thermometer, with a *gilt bulb*, beside the first. The naked glass thermometer—a good radiator—remained 9° Fahr. colder than its companion. To determine the true temperature of the air is, it may be remarked, a task of some difficulty: a glass thermometer, suspended in air, will not give the temperature of the air; its own power as a radiant or an absorbent comes into play. On a clear day, when the sun shines, the thermometer will be warmer than the air; on a clear night, on the contrary, the thermometer will be colder than the air. We have just seen that the passage of a cloud can raise the temperature of a thermometer 10° in a few minutes. This augmentation, it is manifest, does not indicate a corresponding augmentation

of the temperature of the air, but merely the interception and reflection, by the cloud, of the rays of heat emitted by the thermometer.

Dr. Wells applied his principles to the explanation of many curious effects, and to the correction of many popular errors. Moon blindness he refers to the chill produced by radiation from the eyes, the shining of the moon being merely an accompaniment to the clearness of the atmosphere. The putrefying influence ascribed to the moonbeams is really due to the deposition of moisture, and bacterial germs, on the exposed animal substances. The nipping of tender plants by frost, when the air of the garden is some degrees above the freezing temperature, is also to be referred to chilling by radiation. A cobweb screen would be sufficient to preserve them from injury.[1]

Wells was the first to explain the formation, artificially, of ice in Bengal, where the substance is never formed naturally. Shallow pits are dug, which are partially filled with straw, and on the straw flat pans containing water are exposed to the clear firmament. The water is a powerful radiant, and sends off its heat copiously into space. The heat thus lost cannot be supplied from the earth—this source being cut off by the non-conducting straw. Before sunrise a cake of ice is formed in each vessel. This is the explanation of Wells, and it is, no doubt, the true one. I think, however, it

[1] With reference to this point we have the following beautiful passage in the Essay of Wells:—'I had often, in the pride of half-knowledge, smiled at the means frequently employed by gardeners to protect tender plants from cold, as it appeared to me impossible that a thin mat or any such flimsy substance, could prevent them from attaining the temperature of the atmosphere, by which alone I thought them liable to be injured. But when I had learned that bodies on the surface of the earth become, during a still and serene night, colder than the atmosphere, by radiating their heat to the heavens, I perceived immediately a just reason for the practice which I had before deemed useless.'

needs supplementing. It appears, from the description, that the condition most suitable for the formation of ice, is not only a clear air, but a *dry* air. The nights, says Sir Robert Barker, most favourable for the production of ice, are those which are clearest and most serene, and *in which very little dew appears after midnight.* The italicised phrase is very significant. To produce the ice in abundance, the atmosphere must not only be clear, but it must be comparatively free from aqueous vapour. When the straw on which the pans were laid became wet, it was always changed for dry straw; and the reason Wells assigned for this was, that the straw, by being wetted, was rendered more compact and efficient as a conductor. This may have been the case, but it is also certain that the vapour rising from the wet straw, and overspreading the pans like a screen, would check the chill, and retard the congelation.

With broken health Wells pursued and completed this beautiful investigation; and, on the brink of the grave, he composed his Essay. It is a model of wise inquiry and of lucid exposition. He made no haste, but he took no rest till he had mastered his subject, looking stedfastly into it until it became transparent to his gaze. Thus he solved his problem, and stated its solution in a fashion which renders his work imperishable.[1]

The theory of dew furnished a signal example of the capacity of Wells as an investigator; and he gave other proofs of this capacity. In 1813 he read before the Royal Society a paper in which 'he distinctly recognised the principle of Natural Selection.' These are the words of Mr. Darwin, who adds, that 'this is the first recognition of the principle that has been indicated.' It gave me

[1] The tract of Wells is preceded by a personal memoir written by himself which has the solidity of an essay of Montaigne.

lively gratification to alight upon this additional proof of the penetration of a favourite author.

Since the time of Wells, various experimenters have occupied themselves with the question of nocturnal radiation; but, though valuable facts have been accumulated, if we except a supplement contributed by Melloni, nothing of importance has been added to the theory of Wells. Mr. Glaisher, M. Martins, and others, have illustrated the subject. The following table contains some results obtained by Mr. Glaisher, by exposing thermometers at different heights above the surface of a grass field. The chilling observed when the thermometer was exposed on long grass, is represented by the number 1000; while the succeeding numbers represent the relative chilling of the thermometers placed in the positions indicated:—

	Radiation.
Long grass	1000
One inch above the points of the grass .	671
Two inches " " .	570
Three inches " " .	477
Six inches " " .	282
One foot " " .	129
Two feet " " .	86
Four feet " " .	69
Six feet " " .	52

It may be asked why the thermometer, which is a good radiator, is not, when suspended in the air, just as much chilled as at the earth's surface. Wells has answered this question. It is because the thermometer, when chilled, cools the air in immediate contact with it; this air contracts, becomes heavy, and descends, thus allowing its place to be taken by warmer air. In this way the free thermometer is prevented from falling very low beneath the temperature of the air. Hence, also, the necessity of a still night for the copious formation of dew; for, when the wind blows, fresh air continually circulates amid the

blades of grass, and prevents any considerable chilling by radiation.

When a radiator is exposed to a clear sky, it tends to keep a certain thermometric distance, if the term may be used, between its temperature and that of the surrounding air. This distance will depend upon the energy of the radiator, but it is to a great extent independent of the temperature of the air. Thus M. Pouillet has proved that in the month of April, when the temperature of the air was 3·6° C., swansdown fell by radiation to —3·5°; the whole chilling, therefore, was 7·1°. In the month of June, when the temperature of the air was 17·75° C., the temperature of the radiating swansdown was 10·54°; the chilling, by radiation, is here 7·21°, almost precisely the same as that which occurred in April. Thus, while the general temperature varies within wide limits, the *difference* of temperature between the radiating body and the surrounding air remains sensibly constant.

These facts enabled Melloni to make an important addition to the theory of dew. He found that a glass thermometer, placed on the ground, is never chilled more than 2° C., or 3·6° F., below an adjacent thermometer, with silvered bulb, which hardly radiates at all. These 2° C., or thereabouts, mark the thermometric distance above referred to, which the glass tends to preserve between itself and the surrounding air. But Six, Wilson, Wells, Parry, Scoresby, Glaisher, and others, have found differences of more than 10° C., or 18° F., between a thermometer on grass, and a second thermometer hung a few feet above the grass. How is this to be accounted for? Very simply, according to Melloni, thus: The grass blades first chill themselves, by radiation, 2° C. below the surrounding air; the air is then chilled by contact with the grass, and forms around it a cold aërial bath. But the tendency of the

grass is to keep the above constant difference between its own temperature and that of the surrounding medium. It therefore sinks lower. The air sinks in its turn, being still further chilled by contact with the grass; the grass, however, seeks to re-establish the former difference; it is again followed by the air, and thus, by a series of interactions, the entire stratum of air in contact with the grass becomes lowered to a temperature far below that which corresponds to the actual radiative energy of the grass.

Many futile attempts have been made to detect the warmth of the moon. No doubt every luminous ray is also a heat-ray; but the light-giving power is not even an approximate measure of the calorific energy of a beam. With a large polyzonal lens, Melloni converged an image of the moon upon his pile; but he found the cold of his lens far more than sufficient to mask the heat thus produced. He screened off his lens from the heavens, placed his pile in the focus of the lens, waited until the needle came to zero, and then removing his screen, allowed the concentrated light to strike his pile. The slight air-draughts of the place were sufficient to disguise the effect. He then stopped the tube in front of his pile with glass screens, through which the light went freely to the instrument, where it was converted into heat. *This heat could not get back through the glass screen*, and thus Melloni, imitating De Saussure, accumulated his effects, and obtained a galvanometric deflection of 3° or 4° of heat.

By far the greater part of the heat emitted by the full moon must consist of obscure rays, and these are almost wholly absorbed by our atmospheric vapour. Even such rays as might be able to cross the earth's atmosphere would be utterly cut off by such a lens as Melloni made use of. It might be worth while to make the experiment with a metallic reflector, instead of with a lens. I have myself

tried a conical reflector of very large dimensions, but have hitherto been defeated by the unsteadiness of London air [1]

THE SUN.

We have now to turn our thoughts to the source from which terrestrial and lunar heat is almost wholly derived. This source is the sun; for if the earth has ever been a molten sphere, which is now cooling, the quantity of heat reaching its surface from within has long ceased to be sensible. First, then, let us inquire what is the constitution of this wondrous body, to which we owe both light and life.

We will approach the subject gradually, preparing our minds, by previous discipline, for the treatment of so great a problem. You already know how the spectrum of the electric light is formed. Such a spectrum is now upon the screen, with all its magnificent gradations of colour, one passing into the other, without solution of continuity. The light from which this spectrum is derived, is emitted from the solid incandescent carbon-points within our electric lamp. All other white-hot solids give a similar spectrum. When, for example, a platinum wire is heated to whiteness by an electric current, and when its light is examined by a prism, the same gradations of colour are found, no gap whatever existing between one colour and another. But by intense heat—by the heat of the electric lamp, for example—we can volatilise a metal, and throw upon the screen, not the spectrum of the incandescent solid, but of its incandescent vapour. The spectrum is completely changed; instead of being a continuous grada-

[1] With his great reflecting telescope Lord Rosse has since treated this question exhaustively. He determined the radiant heat of the moon, not only when full, but during its various phases.

tion of colours, it consists of a series of brilliant lines, separated from each other by spaces of darkness.

The lower piece of carbon here employed is a cylinder, about half an inch in diameter, in the top of which is scooped a small hollow. Into this hollow is put a piece of zinc. When the upper carbon-point is brought down upon the zinc the current passes; and when the points are afterwards drawn apart, a stream of purple light passes between them. That coloured stream, the magnified image of which is fully eighteen inches long, is zinc vapour; it contains the molecules of the zinc discharged across from carbon to carbon. These are now oscillating in certain definite periods, and the colour which we perceive is the composite impression produced by their oscillations.

Resolving, by a prism, the light of the arc into its component colours, we have no longer a continuous spectrum, but splendid bands of red and blue light.

I interrupt the current, remove the zinc, and put in its place a piece of copper. On forming the arc we obtain a stream of green light, which can be analysed like the purple light of the zinc. In the spectrum of the copper we have bands of brilliant green, which were absent in the case of zinc. We may therefore infer, with certainty, that the atoms of copper, in the voltaic arc, vibrate in periods different from those of zinc. Let us now inquire how these different vibrations affect each other, when we operate upon a substance composed of zinc and copper,—the familiar substance brass. Its spectrum is now before you, and if you have retained the impression made by our last two experiments, you will recognise in this spectrum the superposition of the two separate spectra of zinc and copper. The alloy emits, without confusion, the rays peculiar to both the metals of which it is composed.

Every metal emits its own system of bands, which are as characteristic as those other physical and chemical qualities which give it its individuality. By a method of experiment sufficiently refined, we can measure, accurately, the position of the bright lines of every known metal. Acquainted with such lines, we should, by the mere inspection of the spectrum of any single metal, be able at once to declare its name. Not only so, but in the case of a mixed spectrum we should be able to declare the constituents of the mixture from which it emanated. From the exhibition of unknown lines, the existence of new metals has been inferred. Bunsen and Kirchhoff, for example, thus discovered Cæsium and Rubidium; and Mr. Crookes, by the same method, discovered Thallium, which gives us a single line of brilliant green.

This law is true, not only of the metals themselves, but also of their compounds, if they be volatile. I place a bit of sodium on the lower cylinder, and cause the voltaic discharge to pass from it to the upper carbon-point; the resultant spectrum yields a band of brilliant yellow. With greater delicacy of experiment that band might be divided into two, with a narrow dark interval between them. A still greater amount of precision would further subdivide the yellow band. Let us now remove the sodium from the lamp and put in its place a little common salt, or chloride of sodium. At this high temperature the sodium is liberated, and it produces the exact yellow band yielded by the pure metal. Thus, also, from the chloride of strontium, we obtain the bands of the metal strontium; and by means of the chlorides of calcium, magnesium, and lithium, we produce the spectra of these respective metals.

Displacing our carbon cylinder by another perforated with holes, into which is crammed a mixture of all the compounds just mentioned, we obtain all the correspond-

ing spectra. Each substance gives out its own peculiar rays, which cut the spectrum into transverse bars of richly coloured light. Having previously made yourselves acquainted with the lines emitted by all the metals, taken separately, you would be able to unravel this composite spectrum, and to name the metals concerned in its production.

The voltaic arc is here employed simply because its light is so intense as to be visible to a large audience like the present; but the same experiments might be made with a common blowpipe flame. The introduction of sodium, or chloride of sodium, turns the flame yellow; strontium turns it red; copper, green, &c. The flames thus coloured, when examined by a prism, show, in general, the exact bands which have been displayed before you.[1]

We have here, then, the *radiation* of definite groups of rays by incandescent vapours. Let us now turn our attention to the *absorption* of definite groups of rays by gaseous substances. A famous experiment of Sir David Brewster's, thrown into a form suited to the lecture-room, will illustrate this power of selection. Into a cylinder, whose ends are stopped by plates of glass, is introduced a quantity of nitrous acid gas, the presence of which is indicated by its rich brown colour. Projecting a brilliant spectrum on the screen, and placing the cylinder, containing the brown gas, in the path of the beam as it issues from the lamp, the continuous spectrum is seen furrowed by numerous dark bands. The rays answering to these bands are intercepted by the nitric gas, while it permits the intervening bands of light to pass without hindrance.

[1] The splendid blue band of Lithium was discovered by means of the electric lamp on the occasion here referred to; showing, in opposition to the belief previously entertained, that the number of the bands is not independent of temperature. This subject has, of late, received important expansions.

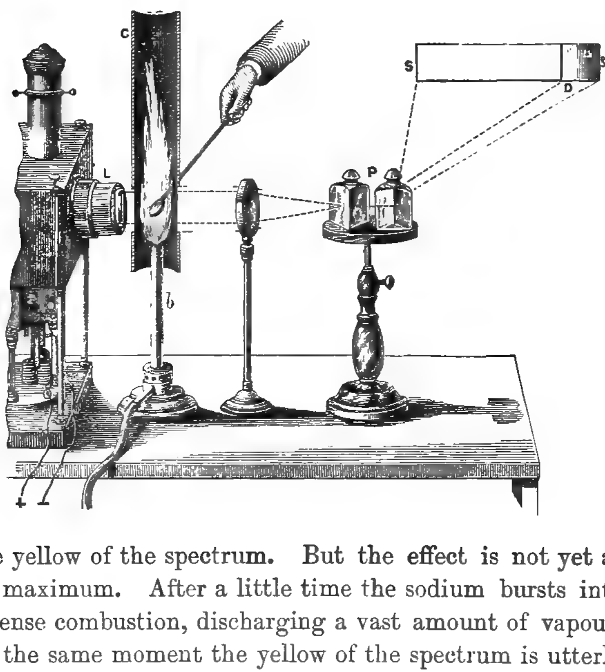
We now come to the great principle on which these phenomena depend, and which we have already, in part, illustrated by our experiments on the radiation and absorption of heat by gaseous matter. This principle, first fully announced by Professor Kirchhoff, is, that *a gas, or vapour, absorbs those precise rays which it can itself emit.* Atoms which swing at a certain rate intercept waves which swing at the same rate. The atoms which vibrate red light will stop red light; the atoms that vibrate yellow will stop yellow; those that vibrate green will stop green, and so of the rest. Absorption, you already know, is a transference of motion from the ether to the molecules immersed in it, and the absorption of any atom is exerted chiefly upon those waves which arrive in periods coincident with its own rate of oscillation.

Let us endeavour to prove this experimentally. We already know that a sodium-flame, when analysed, gives a brilliant band of yellow. This shallow tin vessel contains a mixture of alcohol and water; when the mixture is warmed, its vapour can be ignited, and it then gives a flame so feebly luminous as to be scarcely visible. By mixing salt with the liquid, and again igniting it, the flame, which a moment ago was scarcely to be seen, becomes a strong yellow. Projecting a continuous spectrum upon the screen, I place in the track of the beam, as it issues from the electric lamp, the yellow sodium-flame. If you observe the spectrum narrowly, you will see, in the yellow, a flickering grey band, very faint, but sufficient to show that the flame has, at least in part, intercepted the yellow of the spectrum: it has partially absorbed the precise light which it can itself emit.

But the effect can be made much plainer. Abandoning the salt-flame, I place the intensely hot flame of a Bunsen's burner, *b*, fig. 124, in front of the lamp, so that the beam, whose decomposition is to form our spectrum, shall pass

through the flame. In a little spoon of platinum-foil I place a bit of the metal sodium, about the size of a pea. After forming the spectrum by the prisms P, I introduce the platinum spoon, containing the sodium, into the Bunsen flame. The sodium instantly colours the flame intensely yellow, and already a shadow is seen coming over

FIG. 124.



the yellow of the spectrum. But the effect is not yet at its maximum. After a little time the sodium bursts into intense combustion, discharging a vast amount of vapour. At the same moment the yellow of the spectrum is utterly abolished, a bar of intense darkness, D, taking its place.[1] On withdrawing the sodium, the yellow reappears upon the

[1] Before trying the combustion of the metal, I had tried the salt-flame in a trough ten feet long: the effect, however, is far inferior to that attained by the combustion of the metal. The experiment was first made during my preparations for a Friday evening discourse on the 'Physical Basis of Solar Chemistry,' given in June, 1861.

screen; on reintroducing it, the band is again cut out. That may be done ten times in succession, and in the whole range of optics there is scarcely a more striking experiment. We have thus conclusively proved that the light which the sodium-flame absorbs is the light which it can emit.

Let us be still more precise. The yellow of the spectrum spreads over a certain interval, and we have now to examine whether it is not the particular portion of the yellow emitted by the sodium, that is absorbed by its flame. I place a little brine on the ends of the carbon-points; the continuous spectrum is now seen with the yellow band of the sodium brighter than the rest of the yellow. When the sodium-flame is placed in front, that particular band, which now stands out from the spectrum, is cut away.

You have already seen a spectrum derived from a mixture of various substances, and composed of a succession of sharply defined and brilliant bars. Supposing the composite vapour which produces these bars placed in the path of a beam producing a continuous spectrum, we should cut out of the latter the precise rays emitted by the components of the mixture. We should thus, instead of furrowing the spectrum by a single dark band, as in the case of sodium, furrow it by a series of dark bands, equal in number to the bright bands, produced by the mixture itself, when employed as a source of light.

We now possess knowledge sufficient to enable us to rise to the level of one of the most remarkable generalisations of our age. When the light of the sun is properly decomposed, the spectrum is seen furrowed by innumerable *dark* lines. A few of these were observed for the first time by Dr. Wollaston; but they were investigated with profound skill by Fraunhofer, and called, after him, Fraunhofer's lines. It had long been supposed that these dark bands were, in some way, due to the absorp-

tion of the light which corresponds to them, by the atmosphere of the sun; but nobody knew how. Having once proved that an incandescent vapour absorbs the precise rays which it can itself emit, and knowing that the body of the sun is surrounded by an incandescent photosphere, the supposition at once flashes on the mind, that this flaming envelope may cut off those rays of the central incandescent orb, which the photosphere itself can emit. We are thus led to a theory of the constitution of the sun, which renders a complete account of the lines of Fraunhofer.

The sun, according to Kirchhoff, consists of a central orb, molten or solid, of exceeding brightness, which emits all kinds of rays, and would therefore, if unhindered, give a continuous spectrum. The radiation from the nucleus, however, has to pass through the photosphere, and this vaporous envelope cuts off those particular rays of the nucleus which it can itself emit—the lines of Fraunhofer marking the position of the failing rays. Could we abolish the central orb, and obtain the spectrum of the gaseous envelope alone, we should obtain a striped spectrum, each bright band of which would coincide with one of Fraunhofer's dark lines. These lines, therefore, are spaces of *relative*, not of absolute darkness; upon them the rays of the absorbent photosphere fall; but these, not being sufficiently intense to make good the light intercepted, the spaces which they illuminate are dark, in comparison to the general brilliancy of the spectrum.

It has long been supposed that the sun and planets have had a common origin, and that hence the same substances are common to them all. Can we then detect the presence of any of our terrestrial substances in the sun? We have learned that the bright bands of a metal are characteristic of the metal; that we can, without

seeing the metal, declare its name from the inspection of its bands. The bands are, so to speak, the *voice* of the metal declaring its presence. Hence, if any of our terrestrial metals be contained in the sun's atmosphere, the dark lines which they produce ought to coincide exactly with the bright lines emitted by the vapour of the metal itself. About sixty bright lines have been determined as belonging to the single metal iron. If the light from the incandescent vapour of iron, obtained by passing electric sparks between two iron wires, be allowed to pass through one half of a fine slit, and the light of the sun through the other half, the spectra from both sources of light may be placed one underneath the other. When this is done, it is found that for every bright line of the iron spectrum there is a dark line of the solar spectrum. Reduced to actual calculation, this means that the chances are more than 1,000,000,000,000,000,000 to 1, that iron is in the atmosphere of the sun. Comparing in the same manner the spectra of other metals, Professor Kirchhoff, to whose genius we owe this splendid generalisation, finds iron, calcium, magnesium, sodium, chromium, and many other metals, in the solar atmosphere.

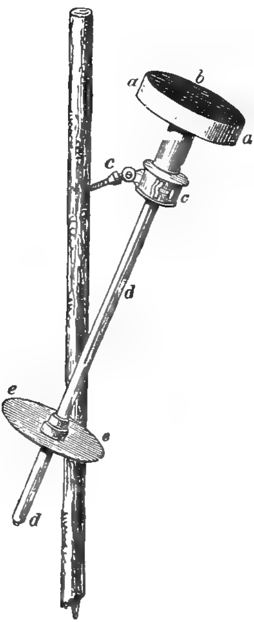
We can imitate, in a way more precise than that hitherto employed, the solar constitution here supposed. In the electric lamp is placed a cylinder of carbon about half an inch thick; and round its upper edge a ring of sodium, the central portion of the cylinder being left clear. I bring down the upper carbon-point upon the middle of the cylinder, thus producing the ordinary electric light. Its proximity to the sodium is sufficient to volatilise the latter, and thus the little central sun is surrounded with an atmosphere of sodium vapour, as the real sun is surrounded by its photosphere. The yellow band is absent in the spectrum of this light.[1]

[1] At this time (June 1861) the experiment of placing a bit of sodium

QUANTITY OF SOLAR HEAT.

The quantity of heat emitted by the sun has been measured by Sir John Herschel at the Cape of Good Hope, and by M. Pouillet in Paris. The agreement between the measurements of both is very remarkable. Sir John Herschel found the direct heating effect of a vertical sun, at the sea level, to be competent to melt 0·00754 of an inch of ice per minute; while, according to M. Pouillet, the quantity is 0·00703 of an inch. The mean of the determinations cannot be far from the truth; and this gives 0·00728 of an inch of ice per minute, or nearly half an inch per hour. Before you (fig. 125) I have placed an instrument, similar in form to that used by M. Pouillet, and called by him a pyrheliometer. The particular instrument which you now see is composed of a shallow cylinder of steel *a a*, filled with mercury. Into the cylinder is introduced the thermometer, *d*, the stem of which is protected by a piece of brass tubing. The flat end of the cylinder is to be turned towards the sun, and the surface thus presented is coated with lampblack. By means of a collar and screw, *c c*, the instrument may be attached to a stake

FIG. 125.



like the bit of zinc and copper already referred to, on the top of the lower cylinder of the lamp, was repeatedly made, the dark band being produced. The form described above was given to the experiment, simply to render its resemblance to the effect of the solar atmosphere more apparent.

driven into the ground, or into the snow, if the observations are made at considerable heights. It is necessary that the surface which receives the sun's rays should be perpendicular to them, and this is secured by attaching to the brass tube which shields the stem of the thermometer, a disk, *e e*, of precisely the same diameter as the steel cylinder, *a a*. When the shadow of the cylinder accurately covers the disk, we are sure that the rays fall, as perpendiculars, on the upturned surface of the cylinder.

The observations are made in the following manner:—First, the instrument is permitted, not to receive the sun's rays, but, to radiate its own heat for five minutes against an unclouded part of the firmament; the decrease of the temperature of the mercury consequent on this radiation being noted. Next, the surface is turned towards the sun, so that the solar rays fall perpendicularly upon it for five minutes—the augmentation of temperature being noted. Finally, the instrument is turned again towards the firmament, away from the sun, and allowed to radiate for another five minutes, the sinking of the thermometer being noted as before. You might, perhaps, suppose that exposure to the sun alone would be sufficient to determine the heating power; but we must not forget, that during the whole time of exposure to the sun's action, the blackened surface of the cylinder is also radiating into space; it is not therefore a case of pure gain: the heat received from the sun is, in part, thus wasted, even while the experiment is going on; and to find the quantity thus lost, the first and last experiments are needed. In order to obtain the whole heating power of the sun, we must add the quantity lost during the time of exposure, and this quantity is the mean of the first and last observations. Supposing the letter R to represent the augmentation of temperature by five minutes' exposure to the sun, and that t and t' represent the reductions of

temperature observed before and after, then the whole force of the sun, which we may call T, would be thus expressed:

$$T = R + \frac{t + t'}{2}$$

The area of the surface on which the sun's rays here fall is known; the quantity of mercury within the cylinder is also known; hence, we can express the effect of the sun's heat upon a given area, by stating that it is competent, in five minutes, to raise so much mercury or so much water, so many degrees in temperature. Water, indeed, instead of mercury, was used in M. Pouillet's pyrheliometer.

The observations were made at different hours of the day, and consequently, through different thicknesses of the earth's atmosphere; augmenting from the minimum thickness at noon, up to the maximum at 6 P.M., which was the time of the latest observation. It was found that the solar energy diminished, according to a certain law, as the thickness of the layer of air increased; and from this law M. Pouillet was enabled to infer that the absorption, if the rays were directed downwards to his instrument from the zenith, would be 25 per cent. of the whole radiation. Doubtless, this absorption would be chiefly exerted upon the longer undulations emitted by the sun; the aqueous vapour of our air, not the air itself, being the principal agent. Taking into account the whole terrestrial hemisphere turned towards the sun, the amount intercepted by the atmospheric envelope is four-tenths of the entire radiation. Thus, were the atmosphere removed, the illuminated hemisphere of the earth would receive nearly twice the amount of heat from the sun that now reaches it.

The total amount of solar heat received by the earth in a year, if distributed uniformly over the earth's surface, would be sufficient to liquefy a layer of ice 100 feet thick, covering the whole earth. The heat of the sun, if used

to melt a stratum of ice applied to the sun's surface, would liquefy it at the rate of 2,400 feet an hour. It would boil per hour 700,000 millions of cubic miles of ice-cold water. Expressed in another form, the heat given out every hour by the sun is equal to that which would be generated by the combustion of a layer of coal, ten feet thick, entirely surrounding the sun; hence the heat emitted in a year is equal to that which would be produced by the combustion of a layer of coal seventeen miles in thickness.

This, then, is the expenditure which has been going on for ages, without our being able, in historic times, to detect the loss. When the tolling of a bell is heard at a distance the sonorous vibrations are quickly wasted, and renewed strokes are necessary to maintain the sound. Like the bell,

Die Sonne tönt nach alter Weise.

But how are its tones sustained? How is the perennial loss made good? We are apt to overlook the wonderful in the common. Possibly to many of us—and even to some of the most enlightened among us—the sun appears as a fire, differing from our terrestrial fires only in the magnitude and intensity of its combustion. But what is the burning matter which can thus maintain itself? All that we know of cosmical phenomena declares our brotherhood with the sun—affirms that the same constituents enter into the composition of his mass as those already known to chemistry. But no earthly substance with which we are acquainted—no substance which the fall of meteors has landed on the earth—would be at all competent to maintain the sun's combustion. The chemical energy of such substances would be too weak, and their dissipation too speedy. Were the sun a block of burning coal, and were it supplied with oxygen sufficient for the

observed emission, it would be utterly consumed in 5,000 years. On the other hand, to imagine it a body originally endowed with a store of heat—a hot globe now cooling—necessitates the ascription to it of qualities wholly different from those possessed by terrestrial matter. If we knew the specific heat of the sun, we could calculate its rate of cooling. Assuming the specific heat to be the same as that of water—the terrestrial substance which possesses the highest specific heat—at its present rate of emission, the entire mass of the sun would cool down 15,000° Fahr. in 5,000 years. In short, if the sun be formed of matter like our own, some means must exist of renewing its wasted power.

METEORIC THEORY OF THE SUN.

I have already alluded to a theory of solar heat first enunciated and developed by Dr. Mayer, which, however bold it may at first sight appear, deserves our serious attention. A special lecture will be devoted to the labours of the extraordinary man here referred to. Meanwhile a brief statement of his theory of solar heat finds its natural position here. Besides comets, planets, and moons, a numerous class of smaller bodies belong to our system which, like the planets and the comets, obey the law of gravitation, and revolve in elliptic orbits round the sun. It is they which, when they come within the earth's atmosphere, and are fired by friction, appear to us as meteors and falling stars.

It is easy to calculate both the maximum and the minimum velocity imparted by the sun's attraction to an asteroid circulating round him. The maximum is generated when the body approaches the sun from an infinite distance; the *entire pull* of the sun being then exerted upon it. The minimum is that velocity which would barely

enable the body to revolve round the sun close to his surface. The final velocity of the former, just before striking the sun, would be 390 miles a second, that of the latter 276 miles a second. The asteroid, on striking the sun, with the former velocity, would develope more than 9,000 times the heat generated by the combustion of an equal asteroid of coal; while the shock, in the latter case, would generate heat equal to that of the combustion of upwards of 4,000 such asteroids. It matters not, therefore, whether the substances falling into the sun be combustible or not; their being combustible would not add sensibly to the tremendous heat produced by their mechanical collision.

Here, then, we have an agency competent to restore his lost energy to the sun, and to maintain a temperature at his surface which transcends all terrestrial combustion. In the fall of asteroids we find the means of producing solar light and heat. It may be contended that this showering down of matter necessitates the growth of the sun; it does so, but the quantity necessary to maintain the observed calorific emission for 4,000 years, would defeat the scrutiny of our best instruments. If the earth struck the sun, it would utterly vanish from perception; but the heat developed by its shock would cover the expenditure of a century.

Whatever be the ultimate fate of this theory, it is a great thing to be able to state the conditions which certainly would produce a sun,—to be able to discern in the force of gravity, acting upon dark matter, the source from which the starry heavens *may* have been derived. For whether the sun be produced, and his emission maintained, by the collision of cosmical masses,—whether, as held by Mayer, the internal heat of the earth be the residue of that developed by the impact of cold dark asteroids, or not—there cannot be a doubt as to the competence of the cause assigned to produce the effects

ascribed to it. Solar light and solar heat lie latent in the force which pulls an apple to the ground.

Mayer published his essay in 1848; five years afterwards Mr. Waterston sketched, independently, a similar theory at the Hull Meeting of the British Association. The Transactions of the Royal Society of Edinburgh for 1854 contain an extremely beautiful memoir, by Sir William Thomson, in which Mr. Waterston's sketch is fully developed. He considers that the meteors, which are to furnish stores of energy for our future sunlight, lie principally within the earth's orbit, and that we see them there, as the Zodiacal Light, 'an illuminated shower, or rather tornado of stones.'

Sir William Thomson adduces the following forcible considerations to show the inadequacy of chemical combination to produce the sun's heat. 'Let us consider,' he says, 'how much chemical action would be required to produce the same effects. . . . . Taking the former estimate, 2,781 thermal units Centigrade (each 1,390 foot pounds) or 3,869,000 foot pounds, which is equivalent to 7,000 horse-power, as the rate per second of emission of energy from every square foot of the sun's surface, we find that more than 0·42 of a pound of coal per second, 1,500 lbs. per hour, would be required to produce heat at the same rate. Now if all the fires of the whole Baltic fleet (this was written in 1854) were heaped up and kept in full combustion over one or two square yards of surface, and if the furnace of a globe all round had every square yard so occupied, where could a sufficient supply of air come from to sustain the combustion? Yet such is the condition we must suppose the sun to be in, according to the hypothesis now under consideration. . . . If the products of combustion were gaseous, they would, in rising, check the necessary supplies of fresh air: if they were

solid and liquid (as they might be if the fuel were metallic) they would interfere with the supply of elements from below. In either, or in both ways, the fire would be choked, and I think it may be safely affirmed that no such fire could keep alight for more than a few minutes, by any conceivable adaptation of air and fuel. If the sun be a burning mass it must be more analogous to burning gunpowder than to a fire burning in air; and it is quite conceivable that a solid mass, containing within itself all the elements required for combustion, provided the products of combustion are permanently gaseous, could burn off at its surface all round, and actually emit heat as copiously as the sun. Thus, an enormous globe of gun-cotton might, if at first cold, and once set on fire round its surface, get to a permanent rate of burning, in which any internal part would become heated sufficiently to ignite, only when nearly approached by the burning surface. It is highly probable indeed that such a body might for a time be as large as the sun and give out luminous heat as copiously, to be freely radiated into space, without suffering more absorption from its atmosphere of transparent gaseous products than the light of the sun actually does experience from the dense atmosphere through which it passes. Let us therefore consider at what rate such a body, giving out heat so copiously, would burn away; the heat of combustion would probably not be so much as 4,000 thermal units per pound of matter burned, the greatest thermal equivalent of chemical action yet ascertained falling considerably short of this. But 2,781 thermal units (as found above) are emitted per second from each square foot of the sun; hence there would be a loss of about 0·7 of a pound of matter per square foot per second. . . . . or a layer half a foot thick in a minute, or 55 miles thick in a year. At the same rate continued, a mass as large as the sun is at present would

burn away in 8,000 years. If the sun has been burning at that rate in past time, he must have been of double diameter, of quadruple heating power, and of eight-fold mass only 8,000 years ago. We may therefore quite safely conclude that the sun does not get its heat by chemical action. . . . and we must therefore look to the meteoric theory for fuel.'

The eminent physicist I have just quoted showed at the same time that the conclusion of physical astronomy is against the idea of the meteoric matter being extra-planetary. He therefore inferred that the meteors which supply the sun with heat, had existed long previously within the earth's orbit. But the researches of Le Verrier on the motion of the planet Mercury, though they indicate the existence of such circulating matter round the sun, show it to be small in quantity. Hence Sir William Thomson, in 1862, arrived at the conclusion that if any appreciable portion of the sun's heat be due to the present raining down of meteoric matter, the matter must circulate round the sun close to his surface. But if such matter existed, it is difficult to imagine how bodies so attenuated as comets could escape from the sun without any sensible loss of energy after having passed at a distance from his surface less than one-eighth of his radius. Sir William Thomson therefore concludes, that though the sun was formed by the collision of smaller masses, this collision being demonstrably able to supply us with twenty million years of solar heat at the present rate of emission, the sun's expenditure, though *originated*, is not *maintained* by mechanical impact; the low rate of cooling and the consequent constancy of the emission being now considered by him as due, in great part, to the high specific heat of the matter of the sun.

From the first memoir of Sir William Thomson (1854)

I extract the following interesting data, showing the amount of heat equivalent to the rotation of the sun and the orbital revolutions of the planets, or the amounts of heat which would be generated if a brake were applied at the surface of the sun, so as to stop the motion of rotation, and if the planets were stopped in their orbits; also the heat obtainable from gravitation, or that which would be developed by each of the planets falling into the sun. The quantity of heat is expressed by the time during which it would cover the solar emission.

	Heat of Gravitation, equal to Solar emission for a period of	Heat of Revolution, equal to Solar emission for a period of
Sun		116 years 6 days
Mercury	6 years 214 days	15 „
Venus	83 „ 227 „	99 „
Earth	94 „ 303 „	81 „
Mars	12 „ 252 „	7 „
Jupiter	32240 „	14 „ 144 „
Saturn	9650 „	2 „ 127 „
Uranus	1610 „	71 „
Neptune	1890 „	

Thus, if the planet Mercury, urged by gravitation from its present distance, were to strike the sun, the quantity of heat generated would cover the solar emission for nearly seven years; while the shock of Jupiter would cover the loss of 32,240 years. Our earth would furnish a supply for 95 years. The heat of revolution of the sun and planets, taken together, would cover the solar emission for 134 years; while the total heat of gravitation (that produced by the planets falling into the sun) would cover the emission for 45,589 years.

Helmholtz, the eminent German physiologist, physicist, and mathematician, takes a somewhat different view of the origin and maintenance of solar light and heat. He starts from the nebular hypothesis of Laplace, and assuming the nebulous matter, in the first instance, to have been

of extreme tenuity, he determines the amount of heat generated by its condensation to the present solar system. Supposing the specific heat of the condensing mass to be the same as that of water, then the heat of condensation would be sufficient to raise the temperature 28,000,000° Centigrade. By far the greater part of this heat was wasted, ages ago, in space. The most intense terrestrial combustion that we can command is that of oxygen and hydrogen, and the temperature of the pure oxyhydrogen flame is 8061° C. The temperature of a hydrogen flame burning in air, is 3259° C.; while that of the lime light, which shines with such sunlike brilliancy, is estimated at 2000° C. What conception, then, can we form of a temperature more than thirteen thousand times that of the Drummond light? If our system were composed of pure coal, and burnt up, the heat produced by its combustion would only amount to $\frac{1}{3500}$th of that generated by the condensation of the nebulous matter, to form our solar system. Helmholtz supposes this condensation to continue; that a virtual falling down of the superficial portions of the sun towards the centre still takes place, a continual development of heat being the result. However this may be, he shows by calculation that the shrinking of the sun's diameter by $\frac{1}{10000}$th of its present length, would generate an amount of heat competent to cover the solar emission for 2000 years; while the condensation of the sun from its present mean density to that of the earth, would have its equivalent in an amount of heat competent to cover the present solar emission for 17,000,000 years.

'But,' continues Helmholtz, 'though the store of our planetary system is so immense that it has not been sensibly diminished by the incessant emission which has gone on during the period of man's history, and though the time which must elapse before a sensible change in

the condition of our planetary system can occur is totally beyond our comprehension, the inexorable laws of mechanics show that this store, which can only suffer loss, and not gain, must finally be exhausted. Shall we terrify ourselves by this thought? We are in the habit of measuring the greatness of the universe, and the wisdom displayed in it, by the duration and the profit which it promises to our own race; but the past history of the earth shows the insignificance of the interval during which man has had his dwelling here. What the museums of Europe show us of the remains of Egypt and Assyria we gaze upon with silent wonder, in despair of being able to carry back our thoughts to a period so remote. Still, the human race must have existed and multiplied for ages before the Pyramids could have been erected. We estimate the duration of human history at 6,000 years; but, vast as this time may appear to us, what is it in comparison with the period during which the earth bore successive series of rank plants and mighty animals, but no men? Periods during which, in our own neighbourhood (Königsberg), the amber-tree bloomed, and dropped its costly gum on the earth and in the sea; when in Europe and North America groves of tropical palms flourished, in which gigantic lizards, and, after them, elephants, whose mighty remains are still buried in the earth, found a home. Different geologists, proceeding from different premisses, have sought to estimate the length of the above period, and they set it down from one to nine millions of years. The time during which the earth had generated organic beings is again small, compared with the ages during which the world was a mass of molten rocks. The experiments of Bischof upon basalt show that our globe would require 350 millions of years to cool down from 2000° to 200° Centigrade. And with regard to the period during which the first nebulous masses condensed,

to form our planetary system, conjecture must entirely cease. The history of man, therefore, is but a minute ripple in the infinite ocean of time. For a much longer period than that during which he has already occupied this world, the existence of a state of inorganic nature, favourable to man's continuance here, seems to be secured, so that for ourselves, and for long generations after us, we have nothing to fear. But the same forces of air and water, and of the volcanic interior, which produced former geologic revolutions burying one series of living forms after another, still act upon the earth's crust. They, rather than those distant cosmical changes of which we have spoken, will put an end to the human race; and, perhaps, compel us to make way for new and more complete forms of life, as the lizard and the mammoth have given way to us and our contemporaries.'[1]

Seven-and-forty years ago, the following remarkable passage, bearing upon this subject, was written by Sir John Herschel.[2] 'The sun's rays are the ultimate source of almost every motion which takes place on the surface of the earth. By its heat are produced all winds, and those disturbances in the electric equilibrium of the atmosphere which gives rise to the phenomena of lightning, and probably also to terrestrial magnetism and the Aurora. By their vivifying action vegetables are enabled to draw support from inorganic matter, and become in their turn the support of animals and man, and the source of those great deposits of dynamical efficiency which are laid up for human use in our coal strata. By them the waters of the sea are made to circulate in vapour through the air, and irrigate the land, producing springs and rivers. By them are produced all disturbances of the chemical equilibrium of the elements of nature, which by a series of compositions

[1] Wechselwirkung der Naturkräfte, 'Phil. Mag.' Ser. IV. vol. ix. p. 515.
[2] 'Outlines of Astronomy,' 1833.

and decompositions give rise to new products and originate a transfer of materials. Even the slow degradation of the solid constituents of the surface, in which its chief geological change consists, is almost entirely due, on the one hand, to the abrasion of wind or rain and the alternation of heat and frost; on the other, to the continual beating of sea waves agitated by winds, the results of solar radiation. Tidal action (itself partly due to the sun's agency) exercises here a comparatively slight influence. The effect of oceanic currents (mainly originating in that influence), though slight in abrasion, is powerful in diffusing and transporting the matter abraded; and when we consider the immense transfer of matter so produced, the increase of pressure over large spaces in the bed of the ocean, and diminution over corresponding portions of the land, we are not at a loss to perceive how the elastic force of subterranean fires, thus repressed on the one hand and released on the other, may break forth in points where the resistance is barely adequate to their retention, and thus bring the phenomena of even volcanic activity under the general law of solar influence.'

This fine passage requires but the breath of recent investigation to convert it into an exposition of the law of the conservation of energy, as applied to both the organic and inorganic world. Late discoveries have taught us that winds and rivers have their definite thermal values, and that, in order to produce their motion, an equivalent amount of solar heat has been consumed. While they exist as winds and rivers, the heat expended in producing them has ceased to exist, being converted into mechanical motion; but when that motion is arrested, the heat which produced it is restored. A river, in descending from an elevation of 7,720 feet, generates an amount of heat competent to augment its own temperature 10° Fahr., and this amount of heat was abstracted from the sun, in order

to lift the matter of the river to the elevation from which it falls. As long as the river continues on the heights, whether in the solid form as a glacier, or in the liquid form as a lake, the heat expended by the sun in lifting it has disappeared from the universe. It has been consumed in the act of lifting. But at the moment that the river starts upon its downward course, and encounters the resistance of its bed, the heat expended in its elevation begins to be restored. The mental eye, indeed, can follow the emission from its source; through the ether as vibratory motion; to the ocean, where it ceases to be vibration, and assumes the potential form, among the molecules of aqueous vapour; to the mountain-top, where the heat absorbed in vaporisation is given out in condensation, while that expended by the sun in lifting the water to that elevation is still unrestored. This we find paid back to the last unit— by the friction along the river's bed; at the bottom of the cascades where the plunge of the torrent is suddenly arrested; in the warmth of the machinery turned by the river; in the spark from the millstone; beneath the crusher of the miner; in the Alpine saw-mill; in the milk-churn of the châlet; in the supports of the cradle in which the mountaineer, by water power, rocks his baby to sleep. All the forms of mechanical motion here indicated are simply the parcelling out of an amount of calorific motion, derived originally from the sun; and wherever the mechanical motion is destroyed, or diminished, it is the sun's heat which is restored.

ENERGIES OF PLANTS AND ANIMALS.

We have thus far dealt with the sensible motions and energies which the sun produces and confers; but there are other motions and energies, whose relations are not so obvious. Trees and vegetables grow upon the earth, and when burned they give rise to heat, from which immense

quantities of mechanical energy are derived. What is the source of this energy? Let me try to put the answer into plain words. You see this iron rust, produced by the falling together of the atoms of iron and oxygen; you cannot see this transparent carbonic acid gas, which is formed by the union of carbon and oxygen. The atoms thus united resemble a weight resting on the earth; their mutual attraction is satisfied. But as I can wind up the weight, and prepare it for another fall; even so these atoms can be wound up, separated from each other, and thus enabled to repeat the process of combination.

In the building of plants, carbonic acid is the material from which the carbon of the plant is derived, while water is the substance from which it obtains its hydrogen. The solar rays wind up the weight. They sever the united atoms, setting the oxygen free, and allowing the carbon and the hydrogen to aggregate in woody fibre. If the sun's rays fall upon a surface of sand, the sand is heated, and finally radiates away as much heat as it receives; let the same rays fall upon a forest; then the quantity of heat given back is less than that received, for a portion of the sunlight is invested in the building of the trees. We have already seen how heat is consumed in forcing asunder the atoms of bodies; and how it reappears, when the attraction of the separated atoms comes again into play.[1] The precise considerations which we then applied to heat, we have now to apply to light, for it is at the expense of the solar light that the chemical decomposition takes place. Without the sun, the reduction of the carbonic acid and water cannot be effected; and, in this act, an amount of solar energy is consumed, exactly equivalent to the molecular work done.

Combustion is the reversal of this process of reduction, and all the energy invested in a plant reappears as heat,

[1] Lecture V.

when the plant is burned. I ignite this bit of cotton, it bursts into flame; the oxygen again unites with its carbon, and an amount of heat is given out, equal to that originally sacrificed by the sun to form the bit of cotton. So also as regards the 'deposits of dynamical efficiency' laid up in our coal strata; they are simply the sun's rays in a potential form. We dig from our pits, annually, more than a hundred million tons of coal, the mechanical equivalent of which is of almost fabulous vastness. The combustion of a single pound of coal, in one minute, is equal to the work of three hundred horses for the same time. It would require nearly one hundred and fifty millions of horses, working day and night with unimpaired strength for a year, to perform an amount of work equivalent to the energy which the sun of the Carboniferous epoch invested in one year's produce of our coalpits.

The farther we pursue this subject, the more its interest and its wonder grow upon us. You have learned how a sun may be produced by the mere exercise of gravitating force; that by the collision of cold dark planetary masses the light and heat of our central orb, and also of the fixed stars, may be obtained. But here we find the physical powers, derived or derivable from the action of gravity upon dead matter, introducing themselves at the very root of the question of vitality. We find in solar light and heat the very mainspring of vegetable life.

Nor can we halt at the vegetable world, for it, mediately or immediately, is the source of all animal life. Some animals feed directly on plants, others feed upon their herbivorous fellow-creatures; but all, in the long run, derive life and energy from the vegetable world; all, therefore, as Helmholtz has remarked, may trace their lineage to the sun. In the animal body the carbon and hydrogen of the vegetable are again brought near the oxygen from which they had been divorced, and which is

now supplied by the lungs. Reunion takes place, and animal heat is the result. Save as regards intensity, there is no difference between the combustion that thus goes on within us, and that of an ordinary fire. The products of combustion are in both cases the same, namely, carbonic acid and water. Looking then at the physics of the question, we see that the formation of a vegetable is a process of winding up, while the formation of an animal is a process of running down. This is the rhythm of Nature as applied to animal and vegetable life.

But is there nothing in the human body to liberate it from that chain of necessity which the law of conservation coils around inorganic nature? Look at two men upon a mountain side, with apparently equal physical strength; the one will sink and fail, while the other scales the summit. Has not volition, in this case, a creative power? Physically considered, the law that rules the operations of a steam-engine rules the operations of the climber. For every pound raised by the former, an equivalent quantity of its heat disappears; and for every step the climber ascends, an amount of heat, equivalent jointly to his own weight and the height to which it is raised, is lost to his body. The strong will can draw largely upon the physical energy furnished by the food; but it can *create* nothing. The function of the will is to *apply* and *direct*, not to create.[1]

I have just said that, as a climber ascends a mountain, heat disappears from his body; the same statement applies to animals performing work. It would appear to follow from this, that the body ought to grow colder, in the act of climbing or of working, whereas universal experience proves it to grow warmer. The solution of this seeming contradiction is found in the fact, that when the muscles

[1] See 'Muscular Heat in relation to Work,' p. 83.

are exerted, augmented respiration, and increased chemical action, set in. The fan which urges oxygen into the fire within is more briskly moved; and thus, though heat actually disappears as we climb, the loss is more than covered by the increased activity of the chemical processes.

By means of a modification of the thermo-electric pile, Becquerel and Breschet proved heat to be developed in a muscle when it contracts. Billroth and Fick have also found that in the case of persons who die of tetanus, the temperature of the muscles is sometimes nearly eleven degrees Fahrenheit in excess of the normal temperature. Helmholtz has shown that the muscles of dead frogs, in contracting, produce heat; and an extremely important result as regards the influence of contraction has been obtained by Ludwig and his pupils. Arterial blood, you know, is charged with oxygen: when this blood passes through a muscle in an ordinary uncontracted state, it is changed into venous blood which still retains about $7\frac{1}{2}$ per cent. of oxygen. But if the arterial blood pass through a *contracted* muscle, it is almost wholly deprived of its oxygen, the quantity remaining amounting, in some cases, to only $1\frac{3}{10}$ per cent. Another result of the augmented combustion within the muscles when in a state of activity, is an increase in the amount of carbonic acid expired from the lungs. Dr. Edward Smith has shown, that the quantity of this gas expired during periods of great exertion may be five times that expired in a state of repose.

The grand point permanent throughout all these considerations is, that *nothing new is created in physical nature.* We can make no movement which is not accounted for by the contemporaneous extinction of some other movement. And how complicated soever the motions of animals may be, whatever may be the change which the molecules of our food undergo within our

bodies, the whole energy of animal life consists in the falling of the atoms of carbon and hydrogen and nitrogen from the high level which they occupy in the food, to the low level which they occupy when they quit the body. But what has enabled the carbon and the hydrogen to fall? What first raised them to the level which rendered the fall possible? We have already learned that it is the sun. Not only is the sun chilled, that we may have our external fires, but he is likewise chilled that we may have our internal warmth and our powers of locomotion.

The subject is of such vast importance, and is so sure to tinge the whole future course of philosophic thought, that I will dwell upon it a little longer, and endeavour, by reference to analogical processes, to give you a clearer idea of the part played by the sun in vital actions. We can raise water by mechanical action to a high level; and that water, in descending by its own gravity, may be made to assume a variety of forms, and to perform various kinds of mechanical work. It may be made to fall in cascades, rise in fountains, twirl in eddies, or flow along a uniform bed. It may, moreover, be employed to turn wheels, lift hammers, grind corn, or drive piles. But all the energy exhibited by the water during its descent is merely the parcelling out and distribution of the original energy which raised it up on high. In this precise sense is the energy of man and animals the parcelling out and distribution of an energy originally exerted by the sun.

But the question is not yet exhausted. The water which we used in our first illustration produces all the motion displayed in its descent, but the *form* of the motion depends on the character of the machinery interposed in the path of the water. Thus also the primary action of the sun's rays is qualified by the atoms and molecules among which their power is distributed. Molecular forces deter-

mine the form which the solar energy will assume. In the one case this energy is so conditioned by its atomic machinery as to result in the formation of a cabbage; in another case it results in the formation of an oak. So also as regards the reunion of the carbon and the oxygen in the animal—the form of their reunion is determined by the molecular machinery through which the combining energy acts. In one case the germ determines the formation of a man, in another the formation of a frog. All the philosophy of the present day tends to show that it is the directing and compounding, in the organic world, of forces belonging equally to the inorganic, that constitute the mystery and the miracle of vitality.

In discussing the material combinations which result in the formation of the human organism, it is impossible to avoid taking side glances at the phenomena of consciousness and thought. Science has asked daring questions, and will, no doubt, continue to ask such. Problems will assuredly present themselves to men of a future age, which, if enunciated now, would appear to most people as the direct offspring of insanity. Still, though the progress and development of science may seem to be unlimited, there is a region beyond her reach—a line with which she does not even tend to inosculate. Given the masses and distances of the planets, we can infer the perturbations consequent on their mutual attractions. Given the nature of a disturbance in water, air, or ether, we can infer from the properties of the medium how its particles will be affected. In all this we deal with physical laws, and the mind runs freely along the line which connects the phenomena, from beginning to end. But when we endeavour to pass, by a similar process, from the region of physics to that of thought, we meet a problem not only beyond our present powers, but transcending any conceivable expansion of the powers we now possess. We may

think over the subject again and again, but it eludes all intellectual presentation. The origin of the material universe is equally inscrutable. Thus, having exhausted science, and reached its very rim, the real mystery of existence still looms around us. And thus it will ever loom—ever beyond the bourne of man's intellect—giving the poets of successive ages just occasion to declare that

> We are such stuff
> As dreams are made of, and our little life
> Is rounded by a sleep.

Still, presented rightly to the mind, the discoveries and generalisations of modern science constitute a poem more sublime than has ever yet addressed the human imagination. The natural philosopher of to-day may dwell amid conceptions which beggar those of Milton. Look at the integrated energies of our world,—the stored power of our coal-fields; our winds and rivers; our fleets, armies, and guns. What are they? They are all generated by a portion of the sun's energy, which does not amount to $\frac{1}{2300000000}$ of the whole. This is the entire fraction of the sun's force intercepted by the earth, and we convert but a small fraction of this fraction into mechanical energy. Multiplying all our powers by millions of millions, we do not reach the sun's expenditure. And still, notwithstanding this enormous drain, in the lapse of human history we are unable to detect a diminution of his store. Measured by our largest terrestrial standards, such a reservoir of power is infinite; but it is our privilege to rise above these standards, and to regard the sun himself as a speck in infinite extension—a mere drop in the universal sea. We analyse the space in which he is immersed, and which is the vehicle of his power. We pass to other systems and other suns, each pouring forth energy like our own, but still without infringement of the law, which reveals immutability in the midst of change, which

recognises incessant transference or conversion, but neither final gain nor loss. The energy of Nature is a constant quantity, and the utmost man can do in the pursuit of physical truth, or in the applications of physical knowledge, is to shift the constituents of the never-varying total, sacrificing one if he would produce another. The law of conservation rigidly excludes both creation and annihilation. Waves may change to ripples, and ripples to waves —magnitude may be substituted for number, and number for magnitude—asteroids may aggregate to suns, suns may invest their energy in floræ and faunæ, and floræ and faunæ may melt in air—the flux of power is eternally the same. It rolls in music through the ages, while the manifestations of physical life, as well as the display of physical phenomena, are but the modulations of its rhythm.

LECTURE XVIII.

THE LABOURS OF MAYER.

OUR fifth lecture brought us face to face with one of the most remarkable scientific figures of this age, whose labours were still further referred to in our last lecture. Dr. Julius Robert Mayer died in 1878, and a brief statement of what he accomplished, and of the circumstances of its accomplishment, will give you a more or less coherent image of the man, and constitute a kind of tribute to his memory. His youth was in great part spent in a seminary in Schönthal, near Heilbronn, the desire to be near a comrade causing him to choose a school which had little to do with science, and much with theology. He afterwards studied medicine in the University of Tübingen, from which he was rusticated for an offence so small and venial as to make clear the intolerable pettiness of German officialism at that time. In scientific matters his youth was marked by originality and penetration, while the ancient classics were studied through necessity, and not through love. In 1840 he accepted the post of physician, in a Dutch vessel, bound for Batavia, with a captain and crew of twenty-eight persons. After the voyage the crew were attacked with an acute inflammatory affection of the lungs, for which copious bleeding was resorted to. The blood drawn was of so bright a red as to suggest in the first instance to Mayer that he had struck an artery instead of a vein. This set him pondering on the relationship of external temperature to the

processes of oxidation going on in the human body; and his explanation of the observed effect was that in a hot climate less fuel was consumed, and therefore less alteration of the blood occurred, in keeping up the temperature of the body, than in a cold one. He was thus brought to reflect on the whole physiological theory of respiration, which he pondered until he had extracted from it meanings altogether new. Mayer saw that, inasmuch as the same quantity of fuel produced, under all circumstances, by its perfect combustion, the same amount of heat, then if by friction, or other mechanical action, heat be produced by the living body outside of itself, this heat must be derived from an inner combustion producing no heat. Unless a perpetual motion be possible, the mechanical heat, he reasoned, must be withdrawn from the organism, the internal and external heat making up the total heat of oxidation.

In 1841, after his return from Batavia, we find him discussing the principle of the conservation of energy with his friend Rümelin, now Chancellor of the University of Tübingen.[1] 'We were once walking along the public road,' says Herr Rümelin, 'when the diligence with four steaming horses passed us. "What, in your opinion," asked Mayer, "is the physical effect of the muscular force of these horses?" I replied that I knew of no other effect than the weight of the horses' bodies, of the carriage and what it contained, having suffered a certain displacement in space, which, without such expenditure of force, would not have occurred. "But," said Mayer, "suppose them to pull up half-way and drive back to Heilbronn—what is then the physical effect?" I replied that two displacements in space would then have occurred, the first of which would have been neutralised by the second. Mayer retorted that he could not call this a physical effect.

[1] The author of a brief but deeply interesting account of Mayer's life, published in the 'Augsburger Zeitung.'

It is quite indifferent, he urged, whether the passengers be landed at Heilbronn or at Oehringen —whether any final displacement occurs or not. The translation of the carriage is the motive and incident of the horses' work, but not its physical effect. The heating of the horses, the accelerated inward combustion, the frictional heat of the sliding wheels which had left their marks in blue stripes upon the road, the warmth of the axles—these are not mere accidents. On the contrary, the motion of the horses and their mechanical work are transformed into these phenomena of heat[1]—transformed, moreover, in a constant quantitative ratio, the discovery and formulation of which Mayer regarded as the most important part of the problem he had set before him. Of the correctness of the principle he had not at that time the slightest doubt.' Soon afterwards, 'certainly,' adds Herr Rümelin, 'in 1841,' he wrote his first brief paper entitled 'Bemerkungen über die Kräfte der unbelebten Natur,' which, being on a physical subject, he sent to Poggendorff's Annalen. But Poggendorff would have nothing to do with it, and it was accordingly sent to Giessen, where Liebig, to Mayer's inexpressible gratification, gave it a place in his Annalen early in 1842.

From the outset Mayer laid a firm grasp on the phenomena of vital dynamics, the main effort of his life being directed to bringing them under the yoke of physical investigation. A physical basis was his primary need; and his first paper was therefore devoted to inorganic nature. His second and more mature paper on Organic Motion, published in 1845, may be broadly divided into two parts, in the first of which he deals with the principle of the conservation of energy, and in the second applies it to vital phenomena. At the outset of the paper he an-

[1] Mayer's striking figure, referred to at page 9; wherein he compares the consumption and reproduction of heat by a railway train, to a process of distillation, must have occurred to him about this time.

nounces, as he had previously done in 1842, the convertibility and quantitative constancy of force or energy. His definitions of 'force' were expressly extended so as to include what we now call potential and dynamic energy, the shifting forms of which constituted his theme. Chemistry, he says, deals with the qualitative changes which matter undergoes under different circumstances, the form of the matter, and not its amount, being subject to change. What chemistry does for matter physics must do for force. The force is as unchangeable as the matter, and the function of physics is to study force in all its forms, and to ascertain the conditions of its metamorphoses. This, he says, is the only problem with which natural philosophy has any concern, for the creation or annihilation of force is not only unrealisable but unthinkable.

For thousands of years, he continues, men have employed the powers of inorganic nature to obtain mechanical effects. But to the ancient forces of moving air and of falling water a new force has been added in modern times—the force of *heat*. This also may be converted into mechanical effect. If to a train weighing 100,000 lbs. a velocity of 30 feet a second is to be imparted, this may be accomplished by permitting the train to roll down an incline until the required velocity has been attained. Trains, however, move without any such exercise of gravitating force, and, despite the friction of their parts, they are kept in motion. Let this friction be supposed equivalent to a rising gradient of 1 in 150, then, with a velocity of 30 feet a second, the weight of the train will be lifted 720 feet in an hour, which corresponds to the work of about forty-five horses. This large quantity of work implies the expenditure of an equal amount of power, and, in the case of the locomotive, the power expended is *heat*.

In the working steam-engine there is an absolute loss of heat. The quantity of heat imparted to the steam

is greater than that which can be obtained from its recondensation; the missing quantity being that usefully applied, or converted into mechanical effect. The more perfect the machine the less will be the amount of heat obtainable from the recondensation of the steam. The best engines make the difference about 5 per cent., that is to say, 100 lbs. of coal burnt in a good working engine yield no more heat than 95 lbs. burnt without performing work.

To prove this important proposition, says Mayer, we must investigate the relationship of elastic fluids to heat and to mechanical work. Gay Lussac has proved by experiment that when air which fills a vessel, streams from it into a second exhausted vessel of the same size, the vessel from which the air issues is cooled, while that which it enters is warmed by precisely the same number of degrees.[1] This experiment, which is distinguished for its simplicity, shows that a given weight and volume of air may expand to double or quadruple its volume without experiencing any change of temperature; or, in other words, that *in the simple expansion of the gas, when no work is performed, no heat is consumed.*

Let a cubic inch of air at 0° C., and under a pressure of 28 inches of mercury, be heated at a constant volume to 274°; and let the quantity of heat required to warm this air be x. When it streams into another exhausted recipient of the same volume, the air will retain its average temperature of 274°. A medium surrounding both vessels will suffer no change of temperature. Again let a cubic inch of air, not at constant volume, but under a constant pressure of 28 inches of mercury, be heated from 0° to 274°, a greater quantity of heat will now be needed than before. Let this new quantity be $x + y$. If the air be

[1] Gay Lussac's experiment is referred to at p. 135.

permitted to cool in the two cases, it will give back the heat communicated to it. The air, which, in cooling, is not followed by an external pressure, will, on falling from 274° to 0°, give out the heat x; while that which cools under constant pressure will yield the heat $x+y$.

Steam in an engine, when it expands under a piston, behaves like air expanding under constant pressure. The heat necessary to the expansion of the steam and the performance of the external work, is $X+Y$. When the steam is cooled, the pressure of the piston ceases, or is exercised in a greatly diminished degree. The heat given out in cooling will be X. With every stroke of the piston, therefore, there is the loss of heat Y; that is to say, *a consumption of heat is inseparably connected with the working of the engine.*

From the quantity of fuel consumed the total amount of heat produced may be calculated. The quantity of unconverted heat, however, can only be roughly estimated, hence an approximate determination of that usefully applied is all that can be arrived at in this way. More sharply and simply the problem may be solved by calculating the quantity of heat rendered 'latent' when a gas expands under pressure. Let the amount of heat required to raise the temperature of a gas at constant volume 1° be x, then to raise the gas under constant pressure 1° the heat required will be $x+y$. Let the weight raised in the latter case by the expanding gas be P, and the height to which it is raised h, then we have

$$y = P \times h.$$

That is to say, the excess of heat imparted when the gas performs work is proportional to the weight raised multiplied into the height to which it is raised. Mayer then goes on to calculate the mechanical equivalent of heat in a

manner substantially the same as that adopted in our fifth lecture.[1]

After going formally through this calculation, he immediately bases on it the determination of the useful effect in steam-engines; finding it to be only about 5 per cent. of the consumed fuel. From the velocity imparted to a cannon ball by a given weight of powder, he concludes that in certain cases 9 per cent. of the heat of the consumed charcoal is transferred to the projectile. And here he indicates a *vera causa* for the observation of Rumford referred to in our second lecture (p. 47), where a gun firing a number of balls was found less heated than when firing blank cartridge. He gives various illustrations of the mechanical generation of heat, and describes observations of his own made in a paper-mill, in which four pulping machines, each containing about 80 lbs. of paper and 1200 lbs. of water, were at work. In 32 minutes the pulp rose from 14° to 16° C. The highest temperature attained was 30°, which remained constant for several hours. Assuming that in one minute a horse can raise 27,000 lbs. a foot high, the heating of 1,280 lbs. of pulp and water 1° in 16 minutes (not taking into account the heat communicated to the apparatus) is equivalent to 3·16-horse power. The factory estimate was 5-horse power. Does the mechanical action of the five horses, he asks, become nothing in the machine? Fact answers: *It becomes heat.*

Mayer's comparison of chemical and mechanical processes furnishes a striking illustration of his insight and power of generalisation. A weight so distant from the

[1] In his first calculation Mayer used the specific heat of air as determined by Delaroche and Bérard. He afterwards employed the more correct determination of Regnault. The former determination makes the mechanical equivalent of heat about $\frac{6}{7}$ths of its true value. The numbers first published by Mayer are here preserved.

earth that the attraction of gravitation is insensible, he defines as being in a state of *mechanical separation*, while the falling of the same weight to the earth he defines as a case of *mechanical combination*. Such a mass would reach the earth's surface with a velocity of 34,450 feet a second; and the heat generated by its collision would raise the temperature of its own weight of water 17,356° C. Chemical combination is in principle the same. Thus the chemical combination of one gramme of carbon and 2·6 grammes of oxygen, is equivalent to the mechanical combination of ½ a gramme with the earth. The chemical combination of 1 gramme of hydrogen with 8 grammes of oxygen is nearly equivalent to the mechanical combination of 2 grammes with the earth. The heat developed in both cases would be 34,700 thermal units. If then for small distances and velocities, ordinary mechanical energy dwindles in comparison with the more intense chemical forces, we find the case reversed when we extend our vision to the action of gravity in celestial space.

Among all terrestrial substances the combination of oxygen and hydrogen produces the greatest amount of heat. One gramme of explosive gas yields, on changing into water, 3,850° C. One gramme of a mixture of carbon and oxygen yields by its chemical union 2,370°. But inasmuch as 17,356° C. of heat must be applied to transport a weight of one gramme beyond the earth's sphere of attraction, it follows that no chemical affinity existing upon the earth could furnish an amount of heat adequate to the complete mechanical separation of the body which exerts it from the earth.

From the combination of atoms Mayer ascends to the combination of worlds. 'The earth,' he says, 'moves in its orbit with a mean velocity of 93,700 feet a second. To produce this motion by the combustion of coal, fifteen times the earth's weight would have to be consumed. The heat here

produced would be competent to raise the temperature of the earth's weight of water 128,000° C. A fraction therefore of the energy with which the earth moves in its orbit would suffice to dissolve all mechanical connection between its parts. Supposing the earth to lie at rest upon the surface of the sun; to lift it and place it at the earth's present distance from the sun, and to impart to it there the velocity of 93,700 feet a second, would require 429 times the quantity of coal above mentioned, or, in other words, a weight of that combustible 6,435 times the weight of our world.

He then discusses the five principal forms of energy—gravity, chemical affinity, electricity, magnetism, and heat—and under five-and-twenty separate heads he describes their relations and mutual conversions. Preconceived notions, he says, sanctioned by time and strengthened by the influence of first impressions, but not the verities of nature, are opposed to the propositions here enunciated. He expressly denies materiality to heat and electricity, though knowing that he has against him 'the most deeply rooted convictions, and hypotheses canonised by the greatest authorities.' With the theory of imponderables, he says, we banish from science the last residue of the mythology of Greece; but we also know that nature in her simple truth is greater and more glorious than all the productions of the human hand, and than all the illusions of the creative mind.

After thus clearing his way through the powers of inorganic nature, Mayer turns to vital phenomena, and at once fixes the attention of his readers upon the Sun.

Measured by human standards the sun is an inexhaustible store of physical energy. It is his force which, like a continually wound-up spring, constitutes the source of all terrestrial activity. The vast amount of energy sent

by our planet into space in the form of wave motion would soon bring its surface to the temperature of death. But the light of the sun is an incessant compensation. It is the sun's light converted into heat which sets our atmosphere in motion, raising the waters to clouds, and causing the rivers to flow. The heat generated by friction in the wheels and axles of our wind- and water-mills has been sent to our planet from the sun in the form of vibratory motion.

The principles of vital dynamics are next traced to their foundation. Nature, continues Mayer, undertakes the task of storing up the light which streams earthward—of condensing the most volatile of all powers into a rigid form, and thus preserving it for our use. She has overspread the earth with organisms which while living take into them the solar light, and by the appropriation of its energy generate incessantly chemical forces. These organisms are *plants*. The vegetable world constitutes the reservoir in which the fugitive solar rays are deposited, and rendered ready for useful application. With this economical provision the existence of the human race is also inseparably connected. The reducing action exerted by solar light on both inorganic and organic substances is well known. This reduction takes place most copiously in full sunlight, less copiously in the shade, being entirely absent in darkness, and even in candle-light. The reduction is a conversion of one form of energy into another—of mechanical effect into chemical tension.

The time does not lie far behind us when it was a subject of contention whether, during life, plants possessed the power not only of changing and rearranging the chemical elements, but even of creating them. Facts and experiments seemed to favour this notion, but more accurate investigation has proved it to be untrue. We now know that the sum of the materials appropriated and

excreted, is equal to the total quantity of matter taken up by the plant. A tree, for example, which weighs several thousand pounds, has taken every grain of its substance from its environment.

Plants consume the force of light and store it up as chemical tension. Since the time of De Saussure the action of light has been known to be necessary to the reduction of the carbonic acid. Here, in the first place, we must inquire whether the light which falls upon living plants is applied differently from that which falls upon dead matter; that is to say, whether *cæteris paribus* plants are less warmed by solar light than other bodies equally dark coloured. The results of the observations hitherto made on a small scale seem to lie within the limits of experimental error. On the other hand everyday experience teaches us that solar heat is powerfully moderated by rich vegetation; although plants, on account of the darkness of their leaves, must be better absorbers than the naked earth. If, concludes Mayer, the evaporation from the plants be proved insufficient to account for this cooling action, then the question above thrown out must be answered in the affirmative.

The second question refers to the cause of the chemical tension produced in the plant. This tension is a physical force. *It is the equivalent of the heat produced by the combustion of the plant.* Does this force, then, come from the vital processes, and without the expenditure of some other form of power? The creation of physical energy, of itself hardly thinkable, seems all the more paradoxical when we consider that it is only by the help of the sun's rays that plants can perform their work. By the assumption of such a hypothetical action of 'vital force' all further investigation is cut off, and the application of the methods of exact science to the phenomena of vitality is rendered impossible. Those who hold a notion

so opposed to the spirit of science are carried thereby into the chaos of unbridled phantasy. 'I therefore hope that I may reckon on the reader's assent when I lay down, as an axiomatic truth, *that just as in the case of matter, so also in the case of force, only a transformation, but never a creation, takes place.*'

To the philosophy of vegetable life thus for the first time enunciated, but still, it is to be remembered, given here only in abstract, nothing material has been added during the last five-and-thirty years.

Mayer spread his philosophic net over the whole organised world. The physical force, he says, collected by plants becomes afterwards the property of another class of creatures. Animals feed on combustible substances belonging to the vegetable kingdom, causing them to reunite with the atmospheric oxygen. Parallel to this process of oxidation runs the mechanical work done by the animal, which is the end and aim of its existence. It is evident that, for equal masses and times, the mechanical effects produced by plants are vanishingly small compared with those produced by animals. While, therefore, in the plant the production of mechanical effect plays only a subordinate part, the conversion of chemical tensions into useful effect is the chief characteristic of animal life.

In the animal body chemical forces are perpetually consumed. Ternary and quaternary compounds undergo the most important changes, and are for the most part given off as burnt substances in the form of binary combinations. The relation of these forces to the heat developed is by no means determined with sufficient accuracy. But where our object is simply the establishment of a principle, we may confine ourselves to the heat generated by the combustion of the carbon. When future experiments as to the energy of the chemical processes have

given us additional data, it will be easy so to modify our numerical calculations as to render them accordant with the new facts.

The work done by an animal may be conveniently expressed by a weight raised to a certain height. It is calculated that a horse, by the exercise of his voluntary muscles for eight hours a day, can raise a weight of 27,000 lbs. (Würtemberg) one foot high per minute. This amounts to 12,960,000 lbs. per day. Assuming with Dulong the heat of combustion of carbon to be 8558° C., the mechanical work corresponding to the combustion of one unit of weight is the raising of 9,670,000 such units to a height of one foot. If, therefore, we express by a weight of carbon the quantity of chemical energy which a horse must expend to perform a day's work, we find it to be 1·34 lbs. per day, 0·167 lb. per hour, and 0·0028 lb. per minute. The heat of this carbon goes purely to the production of mechanical effect.

According to current estimates, the work of a strong labourer is $\frac{1}{7}$ of that of a horse. A man who in one day raises a weight of 1,850,000 lbs. to a height of one foot consumes in this work 0·19 lb. of carbon. This, for an hour, (the day reckoned at eight hours) amounts to 0·024 lb.; for a minute, to 0·0004 lb., or 3·2 grains of carbon. A bowler who imparts to an 8 lb. ball a velocity of 30 feet a second, consumes in this effort $\frac{1}{10}$ of a grain of carbon. A man who lifts his own body, weighing 150 lbs., eight feet high consumes in the act 1 grain of carbon. In climbing a mountain 10,000 feet high the consumption (neglecting the heat generated by the shock of the feet against the earth) is 0·155 lb. or 2 oz. 4 drams 50 grains of carbon.

If the materials consumed were applied solely to the performance of work, the quantities of carbon just calculated would suffice for the times mentioned. In reality,

however, besides the production of mechanical effects, there is in the animal body a continuous generation of heat. The chemical force of the food and of the inhaled oxygen is therefore the source of two different forms of power—mechanical motion and heat—and the sum of these two physical effects is the equivalent of the contemporaneous chemical action. Let the quantity of mechanical work performed by an animal in a given time be converted by friction, or by some other means, into heat; add to this the heat generated directly in the animal body in the same time, the sum of both gives us the exact quantity of heat corresponding to the chemical processes.

In the active animal, the chemical changes are much greater than in the resting one. Let the chemical action in the resting animal during a given time be x and in the active one $x+y$. If, during activity, the same quantity of heat were generated as during rest, the additional chemical force y would be the equivalent of the work performed. More heat, however, is generated in the working organism than in the resting one. In the active animal, therefore, we shall have $x+$ a portion of y for heat, the residue of y being converted into mechanical effect.

It by no means follows from Mayer's principles that the working of a muscle will be accompanied by a lowering of its temperature. Muscular action is accompanied by a general exaltation of chemical action, and a corresponding augmentation of heat. Mayer's position is, that taking the chemical action thus exalted into account, it will not produce within the body its whole amount of heat. 'The residue of y' is lost to the body, though the body's temperature is at the same time augmented.[1]

Mayer's next care is to prove that the extra amount of food consumed by the working animal suffices for the per-

[1] The misapprehension of this fact was the origin of Dr. Elam's vigorous critique in the 'Nineteenth Century' of April 1878.

formance of its work. A strong horse, not working, is amply nourished on 15 lbs. of hay and 5 lbs. of oats per day. Were the animal to perform the daily work of lifting a weight 12,960,000 lbs. one foot high, it could not exist on the same nutriment. To keep it in good condition we must add to its food 11 lbs. of oats. The 20 lbs. of food first mentioned, which is proportional to the quantity we have named x, contains, according to Boussingault, 8·074 lbs of carbon. The additional 11 lbs. of oats —which corresponds to our quantity y—contains, according to the same authority, 4·734 lbs. of carbon.

According to Boussingault also, the carbon taken in, is to that excreted in a combustible form as 3938 : 1364·4. Calculating from these data we find x, or the quantity of carbon burnt by the resting animal, to be 5·2766 lbs. and y to be 3·094 lbs. The quantity consumed in mechanical effect is, as before stated, 1·34 lbs., which we will call z.

We have then the following relations:—1. The mechanical effect is to the total consumption as $z : x+y = 0{\cdot}16$ (about $\frac{1}{6}$). 2. The mechanical effect is to the extra consumption of the working animal as $z : y = 0{\cdot}43$ (nearly $\frac{1}{2}$). 3. The amount of heat generated at rest is to that generated during work as $x : x+y-z = 0{\cdot}75$ ($\frac{3}{4}$) [thus, notwithstanding the loss of z, the working animal is the warmest]. So much for a horse. Taking the data obtained by Liebig with the soldiers and prisoners at Giessen, Mayer establishes the following relations for a man:— 1. The mechanical effect is to the total consumption as $95{\cdot}7 : 540 = 0{\cdot}177$ (with the horse it was 0·16). 2. The mechanical effect is to the surplus consumption of the man at work as $95{\cdot}7 : 285 = 0{\cdot}336$ (with the horse it was 0·43). 3. The generation of heat during rest is to the generation during work as $255 : 540 - 95{\cdot}7 = 0{\cdot}57$ (with the horse it was 0·75).

In these calculations Mayer, for the sake of simplicity, confined himself to the consumed carbon of the food. If it be desired, he says, to make the heat of combustion equal to that of the carbon plus the hydrogen, the additional heat of the latter may be set down as $\frac{1}{4}$ that of the carbon. These determinations, he adds, can make no claim to universal validity, as it is evident that work and consumption must vary with the individual constitution and circumstances of life. The foregoing results, however, prove the following propositions to be in harmony with experimental facts:—

1. The extra food consumed by the working organism furnishes an amply sufficient chemical equivalent for the mechanical work performed.

2. The quantity of carbon devoted by a working mammal to the production of mechanical effect hardly amounts to $\frac{1}{5}$ of the total consumption. The remaining $\frac{4}{5}$ are consumed in the generation of heat.

All this was published in 1845. Seven years subsequently, viz. in 1852, one of the most penetrating intellects of our time, not aware of what Mayer had done so long before, wrote as follows:—'An estimate, according to the same principle, is made by the author (Sir William Thomson) regarding the relation between the thermal and the non-thermal mechanical effects produced by a man at work; by which it appears that probably as much as $\frac{1}{6}$ of the whole work of the chemical forces arising from the oxidation of his food during the twenty-four hours may be devoted to the raising of his own weight, by a man walking up hill for eight hours a day; and perhaps even as much as $\frac{1}{4}$ of the work of the chemical forces may be directed to the overcoming of external resistances by a man exerting himself for six hours a day in such operations as pumping. In the former

case there would not be more than $\frac{5}{6}$, and in the latter not more than $\frac{3}{4}$ of the thermal equivalent of the chemical action emitted as animal heat.'[1] Mayer's calculation, which devotes $\frac{4}{5}$ of the chemical forces to the generation of heat, lies between the two figures of Sir William Thomson.

To this unbiassed, and indeed unconscious, testimony as to Mayer's accuracy, penetration, and historic position, may be added the deliberate conclusion of one of the most learned scientific men of this age, the late M. Verdet, who, in a most excellent discourse delivered before the Chemical Society of Paris, expresses himself thus:—'Ces idées introduites pour la première fois dans la science en 1845, par Jules-Robert Mayer, font faire à la physiologie générale un progrès assurément égal au progrès qui est résulté, vers la fin du siècle dernier, des découvertes de Lavoisier et de Senebier sur la respiration.' The import of Mayer's labours must be profound to cause a French philosopher to rank them beside those of Lavoisier. 'Elles ne sont pas d'ailleurs,' continues Verdet, 'demeurées à l'état de pure théorie, et deux séries distinctes d'expériences les ont déjà confirmées de la manière la plus remarquable.' The first of these series was executed by M. Hirn, the second by M. Baclard.

We return to Mayer. For the conversion of chemical force into mechanical effect, animals, he says, are endowed with specific organs—the muscles. Two things are associated with the activity of a muscle—the influence of a motor nerve, and the oxidation. Like the whole organism, the muscle has its psychical, as well as its physical side; it embraces nerve influence and chemical action.

The will of the steersman rules the motion of the

[1] 'Philosophical Magazine,' vol. iv., p. 258.

steamer. Still this spiritual influence, without which the ship would go to pieces on the nearest rocks, directs but moves not. For the motion we need a physical force—the force of coal—wanting which, despite the strongest exercise of will, the ship remains dead.

Mayer follows up this fine passage by determining more exactly the particular part of the organism which is used up in the performance of mechanical work. In his day it was universally supposed that the muscles were drawn upon; but Mayer, with the mechanical equivalent of heat in his possession, soon perceived that the muscle could not sustain this draft upon its substance. He first enunciated the truth, which subsequent investigators have verified, that the muscle is, in the main, the apparatus by which the *transformation* of energy is effected, but that it is not the substance *consumed* in producing the mechanical effect. He shows by calculation what would occur if the muscles constituted the fire-wood of the body. The muscles of a working man whose total weight is 150 lbs., weigh 64 lbs. Subtracting 77 per cent. for water, 15 lbs. of dry combustible matter remain. Let us assume for a moment (though of course it is not to be taken for granted) that the heat-giving power of this mass, which contains 40 per cent. of oxygen and nitrogen, is equal to that of an equal mass of pure carbon; then, if work corresponding to the daily conversion of 0·19 lb. of carbon into mechanical effect, were accomplished at the expense of the muscles, they would be wholly oxidised in eighty days.

This deduction becomes still more evident if we confine ourselves to the work performed by a single muscle—the heart. Assuming with Valentin the quantity of blood sent forward at each systole by the left ventricle to be 150 cubic centimetres; and with Poiseuille that the hydrostatic pressure in the arteries is equal to that of a column of mercury 16 centimetres high; the mechanical

effect produced by the left ventricle at each systole can then be calculated. It is equal to the lifting of a column of mercury, with a base of a square centimetre and a height of 16 centimetres, 150 centimetres high. The weight of such a column is 217 grammes; the effect of a systole is, therefore,

$$= \begin{cases} 325{\cdot}6 \text{ grammes raised 1 metre.} \\ \quad 2 \text{ lbs.} \qquad\qquad ,, \quad 1 \text{ foot.} \end{cases}$$

This is equivalent to 0·887 of a thermal unit, or to the combustion of 0·0001037 gramme of carbon. Assuming the pulse to beat 70 a minute, which for a day would be 100,800 beats, the work performed by the left ventricle in a day is equivalent to the raising of 202,000 lbs. a foot high. This is equal to 89,428 thermal units, which corresponds to the combustion of 10·45 grammes of carbon. According to Valentin, the work done by the right ventricle is half that done by the left. The work of both chambers in a single day is, therefore, equal to the raising of 303,000 lbs. 1 foot high. This corresponds to 134,143 thermal units, or to the combustion of 15·67 grammes of carbon.

Taking the weight of the whole heart to be 500 grammes, and deducting from this 77 per cent. of water, we have remaining 115 grammes of dry combustible matter. Assuming, as before, this matter to be equivalent to pure carbon, it would follow that the entire organ, if it had to furnish the fuel necessary to its mechanical action, would be oxidised in 8 days. Taking the weight of the two ventricles alone as 202 grammes, under the same conditions they would be completely burnt up in 3½ days. The assumption of a rapid combustion and renewal of the normally active muscles is, says Mayer, in open contradiction to physiological and microscopical facts. And the above numerical values of 3½ and 80 days, respectively, prove to

demonstration that no considerable portion of the combustible matter devoted to mechanical work can be derived from the muscular fibres themselves.[1]

It is a pleasure, as well as a duty, to state the extent to which Mayer had been anticipated by others, as regards vital dynamics. In a postscript to a paper in the December number of the 'Philosophical Magazine' for 1843, Dr. Joule writes thus :—'On conversing a few days ago with my friend Mr. John Davies, he told me that he had himself a few years ago attempted to account for that part of animal heat which Crawford's theory had left unexplained, by the friction of the blood in the veins and arteries, but that, finding a similar hypothesis in Haller's "Physiology," he had not pursued the subject further. It is unquestionable that heat is produced by friction, but it must be understood that the mechanical force expended in the friction is a part of the force of affinity, which causes the venous blood to unite with the oxygen, so that the whole heat of the system must still be referred to the chemical changes. But if the animal were engaged in turning a piece of machinery, or in ascending a mountain, I apprehend that, in proportion to the muscular effort put forth for the purpose, a *diminution* of heat evolved in the system by a given chemical action would be experienced.' This citation embraces, I believe, every word that had been published on vital dynamics prior to the appearance of Mayer's essay on 'Organic Motion.' Mayer, I may add, spoke to the end in terms of the most generous appreciation of the labours of Dr. Joule.

[1] I have here given a résumé of less than half of Mayer's Second Memoir. The remainder is for the most part purely physiological. The time, however, may come when I shall be able to bring it also before the English public.

But we have not yet done with this remarkable man. He began by establishing a secure physical basis for the astonishing superstructure which he afterwards raised. He passed from inorganic to organic nature, and brought the vital processes of both plants and animals under the yoke of his *Obersten Grundsatz*—his highest law. With keen vision and sure step, he sought and found the source of all terrestrial energies, living and non-living, in the sacrifice of solar light and the equivalent generation of chemical forces. But he did not halt here. He asked himself—What is the source of this source? Whence does the sun derive his power, and how is his vast emission maintained? It is obvious that the theoretic answer to this question was in his possession in 1845. He then, as we have seen, affirmed that as a source of heat the full play of gravity in celestial space was immensely greater than the most intense terrestrial affinities; he calculated the thermal effect of the act of mechanical combination with the earth—that is to say, the heat produced by the impact of a body animated by the entire pull of gravity, drawing it from a position where the attraction is insensible down upon the earth; he determined the amount of heat requisite to lift the earth from the surface of the sun, to place it at its present distance, and to impress upon it there its orbital motion; proving the requisite heat to be equal to that produced by the combustion of more than six thousand earths of solid coke. The meteoric theory of solar heat was thus obviously in Mayer's hands at least three years before he formally developed and published it.

The memoir in which the theory is developed is entitled 'Beiträge zur Dynamik des Himmels,' 'Contributions to Celestial Dynamics,' which was published in 1848.[1]

[1] Translated by Dr. Debus, 'Philosophical Magazine,' 1863, xxv. 241, 387, 417.

Every incandescent body as it radiates light and heat, diminishes in temperature and luminousness, and finally, unless the loss be repaired, becomes cold and dark. For light, like sound, consists of vibrations which are communicated by the luminous body to a surrounding medium. If the vibratory motion of a string could take place without any resistance, it would oscillate for all time; but in the same degree as the string communicates its vibrations to the surrounding medium, its own motion becomes weaker, until at last it sinks into a state of rest.

The sun, says Mayer, has often, and appropriately, been compared to an incessantly sounding bell. By what means is the power of this orb sustained in undiminished force, so as to enable it to send forth its rays into the universe so continuously? What are the causes which counteract the sun's exhaustion, thus saving the planetary system from darkness and the cold of death?

In his daring attempt to answer this question, to which slight reference was made in our last lecture, Mayer first considers 'the sources of heat.' After naming several, he goes on:—A general law of nature which knows no exception is that, to obtain heat, something must be expended. However it may vary in other respects, this something can always be referred to one of two categories; either it is material expended in a chemical process, or it is some sort of mechanical work. After defining his unit of heat to be the quantity necessary to raise one kilogramme (about 2 lbs.) of water 1°C., he states the number of such units generated by various combustible substances. Charcoal in oxygen yields 7,200 units; superior coal 6,000 units; dry wood from 3,300 to 3,900 units; sulphur 2,700, and hydrogen 34,600. Experiment proves the number of units of heat generated to depend solely upon the quantity of matter consumed, and not on the way in which it is consumed. The same amount of heat is given out whether the

combustion proceed slowly or quickly—in atmospheric air or in pure oxygen. In like manner, as regards the quantity of heat generated, it is a matter of indifference whether a body—say a metal—is burnt in the air, its heat being measured directly, or oxidised in a voltaic battery, while the heat is generated at another place—say in a distant wire through which the current flows. The same law, he continues, holds good for the mechanical production of heat. The amount depends solely on the quantity of power consumed, and is quite independent of the manner in which the power is expended.[1]

Passing from terrestrial sources to the sun's heat, he cites the respective results of Herschel and Pouillet, the one making the quantity which reaches the earth yearly sufficient to melt a layer of ice 29·2 metres in thickness, while the other makes the thickness of the layer 30·89 metres. Every square metre of the surface of the earth, according to Pouillet's measurement, receives 4,408 units of heat from the sun, while the whole earth receives 2,247 billions of units per minute. With the view of obtaining smaller numbers, Mayer calls the quantity of heat necessary to raise a cubic mile of water 1° C. in temperature, a cubic mile of heat, and finds by calculation that 5·5 cubic miles are sent per minute from the sun to the earth. Let the sun be surrounded by a hollow sphere of a radius equal to the mean distance of the earth from the sun, then the base of the cone of solar light which reaches our earth is to the whole surface of the hollow sphere as 1 to 2,300 millions. The total radiation of the sun amounts, therefore, to 12,650 millions cubic miles of heat per minute.

[1] It may be remarked that in his transference of muscular heat to external space, Mayer does not suppose that the action ever reaches the stage of heat in the muscle. The fire, he says, is transferred *in its nascent condition*. It resembles in this respect the heat of the voltaic battery, the external heat never appearing as heat within the cells.

This amazing radiation, says Mayer, unless the loss is by some means made good, ought to rapidly cool even a body of the magnitude of the sun.

Assuming the capacity of the sun for heat to be equal to the highest known on earth, and its loss by radiation to affect uniformly its whole mass, its temperature ought to decrease 1·8° C. yearly. This for the historic time of 5,000 years would amount to 9,000° C. But, continues Mayer, a uniform cooling of so huge a mass cannot take place. On the contrary, if the radiation were produced at the expense of a given store of heat, or radiant power, the sun would soon become covered with a cold crust which would put an end to the radiation. Considering, then, the continued activity of the sun through countless centuries, we may assume with mathematical certainty the existence of some compensating influence to make good its enormous loss.

Is this restoring agency, he asks, a chemical process?

Making the most favourable assumption, that the sun is a lump of coal, every kilogramme of which produces 6,000 units of heat, Mayer calculates that to yield the present solar expenditure such a block, even if provided with the necessary supply of oxygen, would be burnt away in 4,600 years. He considers, and refutes, the notion that the heat of the sun is due to the friction of his periphery against something in space; showing first of all the intrinsic absurdity of the notion, and then proving by calculation that the whole energy of rotation, if converted into heat, would only cover the expenditure of the sun for 180 years.

He finally discerns the cause of the sun's heat in the organisation of the planetary system itself. The movements of celestial bodies in an absolute vacuum would be as uniform as those of a mathematical pendulum, whereas a resisting medium pervading all space would cause the planets to move in ever diminishing orbits, and to fall at

last into the sun. Assuming such a resisting medium, these wandering celestial bodies must have on the periphery of the solar system their cradle, and in its centre their grave. All these bodies in due time plunge with a violent impetus into their common bed. And, since no cause exists without an effect, every one of these cosmical masses, like a weight falling to the earth, will produce by its shock an amount of heat proportional to its *vis viva.*[1]

On the non-existence or the existence of a resisting ether it depends whether the celestial bodies, the planets, the comets and the asteroids move at constant mean distances round the sun, or whether they are constantly approaching him. Scientific men do not doubt the existence of such an ether—a direct proof indeed of the presence of a resisting medium has been furnished by Encke. He found that the comet named after him, which revolves round the sun in the brief period of 1,207 days, shows a regular acceleration of its motion, in consequence of which the period of each succeeding revolution is shortened by about six hours. The shortening of the diameter of the planetary orbits from this cause has been hitherto inappreciable, but it may happen that in the interval during which the mean distance of the earth from the sun would diminish one metre, a small asteroid would travel more than a thousand miles towards the central body.

As cosmical masses stream from all sides in immense numbers towards the sun, it follows that as they approach thereto, they must become more and more crowded together. The conjecture at once arises that the zodiaca light, that nebulous brightness of vast dimensions which surrounds the sun, owes its origin to such closely packed asteroids. However this may be, so much is certain, that this phenomenon is caused by matter which moves according to planetary laws, from which it follows that the whole

[1] Compare this paragraph with the quotation at p. 566.

mass which produces the zodiacal light is continually approaching and falling into the sun. The effect produced by these impinging masses evidently depends on their final velocities, and in order to determine the latter Mayer discusses some of the elements of the theory of gravitation. He determines the maximum velocity of impact corresponding to the fall of a body from an infinite distance, the minimum velocity which corresponds to the revolution of an asteroid close to the surface of the sun, and the intermediate velocity of a body circulating at a distance from the sun. What thermal effect, he asks, corresponds to such velocities? Is it sufficiently great to play an important part in the immense development of solar heat? Applying a formula previously obtained, he finds that the velocity of an asteroid when it strikes the sun may vary from 445,750 to 630,400 metres a second. From this he deduces the calorific effect of impact, and finds it to be from $27\frac{1}{2}$ to 55 million units of heat. An asteroid, therefore, by its fall into the sun, developes from 4,600 to 9,200 times the heat generated by the combustion of an equal mass of coal.

From experiments on the transmission of radiant heat through glass and other transparent bodies, Mayer infers that the temperature of the source from which solar heat is radiated is far higher than the most powerful process of combustion could make it. Other considerations, he says, lead to the same conclusion. If we imagine the sun surrounded by a hollow sphere, the inner surface of this sphere will receive all his radiant heat. At the distance of our earth from the sun, such a sphere would have a radius 215 times as great and an area 46,000 times as large as the sun himself. The luminous and calorific rays, therefore, which strike this spherical surface at right angles retain only $\frac{1}{46,000}$th part of their original intensity. If it be further considered that our atmosphere absorbs a

portion of the solar heat, it is clear that the rays which reach the tropics at noonday can only possess from $\frac{1}{50,000}$th to $\frac{1}{60,000}$th of the energy with which they started. Gathered from a surface of from 5 to 6 square metres, and concentrated in an area of one square centimetre, these rays would produce about the temperature which exists on the sun—a temperature more than sufficient to convert the most refractory metals into vapour.

A correct theory, he says, of the origin of the sun's heat must assign the cause of such an enormous temperature. According to Pouillet, the temperature at which bodies appear intensely white hot is about 1,500° C. One part of hydrogen combines with eight parts of oxygen to form water. Hence one kilogramme of these two gases, mixed in this proportion, would produce 3,850°. Let us compare this with the heat generated by the fall of an asteroid into the sun. Without taking into account the probably low specific heat of the body compared with that of water, we find the heat developed by the asteroid on collision with the solar surface to be from 7,000 to 14,000 times greater than that of the oxyhydrogen mixture. From data like these the extraordinary diathermic energy of the sun's rays, the immense radiation from its surface, and the high temperature in the focus of a reflector are all to be inferred. It matters not whether the nutriment of the sun be combustible or incombustible. As the brightest artificial light appears dark in comparison with solar light, so the mechanical processes of the heavens throw into the shade the most powerful chemical actions.

Mayer next considers the effect of this constant showering down of meteoric matter on the mass of the sun, comparing it to a fine terrestrial rain which sends down in an hour a layer of water one millimetre in thickness. He concludes that the increase of the sun's volume could scarcely be appreciated in historic times. Not quite so

inappreciable would be the increase of the solar mass. This amounts to 2·1 quintillions of kilogrammes; while the mass of cosmical matter annually precipitated on the sun stands to the sun's mass in the relation of 1 to from 21 to 42 millions. Such an augmentation of the weight of the sun ought to shorten the sidereal year from $\frac{1}{42,000,000}$th to $\frac{1}{85,000,000}$th of its length, or from $\frac{3}{4}$ths to $\frac{3}{8}$ths of a second. Mayer here assumes a compensating action for which there seems no warrant. He supposes a loss of substance to be bound up with the origination and propagation of undulatory motion.

He next fixes his attention on Sun-spots, quoting the description of the solar surface given by Sir John Herschel. These changes on the solar surface, he says, evidently point to the action of some external disturbing force; for every moving power resident in the sun ought to exhaust itself by its own action. With regard to the origin of the spots and the faculæ, probable conjectures are all that he presumes to offer. Since gases when free from any solid particles emit even at very high temperatures only a pale transparent light, it is probable that the intense white light of the sun has its origin in the denser parts of his surface.[1] If this be admitted, the sun-spots and faculæ seem to be caused by the disturbances of the fiery molten ocean, by the plunging into it of streams of asteroids. The deeper and less heated parts of this ocean become thus exposed as spots, whereas the elevations form the so-called faculæ.

There is one other consideration connected with the permanence of our present terrestrial conditions, which is well worthy of our attention. Standing upon one of the London bridges, we observe the current of the Thames

[1] The explanation of the lines of Fraunhofer carries along with it the confirmation of the views of Mayer.

reversed and the water poured upwards twice a day. The water thus moved rubs against the river's bed and sides, and heat is the consequence of this friction. The heat thus generated is, in part, radiated into space, and there lost, as far as the earth is concerned. What is it that supplies this incessant loss? Mayer answers—the earth's rotation. Let us imagine the moon fixed, and the earth turning like a wheel from west to east in its diurnal motion. A mountain on the earth's surface, on approaching the moon's meridian, is, as it were, laid hold of by the moon; it forms a kind of handle, by which the earth is pulled more quickly round. But when the meridian is passed, the pull of the moon on the mountain would be in the opposite direction; it would tend to diminish the velocity of rotation as much as it previously augmented it; and thus the action of all fixed bodies on the earth's surface is neutralised.

But suppose the mountain to lie *always* to the east of the moon's meridian, the pull would then be always exerted against the earth's rotation, the velocity of which would be diminished in a degree corresponding to the strength of the pull. *The tidal wave occupies this position.* In consequence of this, the waters of the ocean are, in part, dragged as a brake along the surface of the earth, and as a brake they must diminish the velocity of the earth's rotation.[1] The investigations of Hansen, Adams and Delaunay have established the fact of retardation. Supposing, then, that we turn a mill by the action of the tide, and produce heat by the friction of the millstones; that heat has an origin totally different from the heat produced by another pair of millstones, which are turned by a mountain stream. The former is produced at the expense of the earth's rotation; the latter at the expense of the sun's heat, which lifted the millstream to its source.

[1] Kant had the sagacity to perceive this.

In an article published in 1862 Sir William Thomson referred the primordial energy of the universe to gravitation. 'Created simply as a difference of position of attracting masses, the potential energy of gravitation was the original form of all the energy of the universe.' This closely resembles, if it be not identical with, the doctrine enunciated by Mayer fifteen years earlier. After showing how the molten condition of the earth had been inferred from its spheroidal shape, Mayer proceeds thus:—'Newton's theory of gravitation, while it enables us to tell from the present form of the earth, its state of aggregation in ages past, points out to us at the same time a source of thermal energy powerful enough to produce such a state of aggregation—powerful enough to melt worlds. It teaches us to consider the molten condition of a planet as the result of the mechanical union of cosmical masses, and thus to derive the radiation of the sun and the heat in the bowels of the earth from a common origin.'

In the article just referred to Sir William Thomson continues thus:—'As surely as the weights of a clock run down to their lowest position, from which they can never rise again, unless fresh energy is communicated to them from some source not yet exhausted, so surely must planet after planet creep in, age by age, towards the sun. When each comes within a few hundred thousand miles of his surface, if he is still incandescent, it must be melted and driven into vapour by radiant heat. Nor, if he be crusted over and become dark and cool externally, can the doomed planet escape its fiery end. If it does not become incandescent, like a shooting star, by friction in its passage through his atmosphere, its first graze on his surface must produce a stupendous flash of light and heat. It may be at once, or it may be after two or three bounds, like a cannon-shot ricochetting on a surface of earth or water, the

whole mass must be crushed, melted, and evaporated by a crash, generating in a moment some thousands of times as much heat as a coal of the same size would produce by burning.[1]

But it is from a comparison of Mayer's 'Celestial Dynamics,' published in 1848, with Sir William Thomson's memoir on the 'Mechanical Energy of the Solar System,' published in 1854, that the similarity of thought in two penetrative minds, working under similar conditions, comes most conspicuously forth. Thomson considers and rejects the assumption that the sun is an incandescent body losing heat. Mayer did the same. Thomson considers and rejects the hypothesis that solar heat is due to chemical action. This was also the course pursued by Mayer. Thomson discusses and emphatically embraces the theory that 'the source of energy from which solar heat is derived is undoubtedly meteoric.' This is the theory so clearly enunciated and so fully developed by Mayer. Thomson concludes that the main source of solar light and heat is the tornado of meteors which shed forth the zodiacal light. This was also Mayer's conclusion. The coincidence between the two essays is simply astonishing. As an instance of agreement in a matter of detail take the following:—'A dark body,' says Thomson, 'of dimensions such as the sun in any part of space, might, by entering a cloud of meteors, become incandescent as intensely in a few seconds, as it could in years of continuance of the same meteoric circumstances, and again getting to a position in space comparatively free from meteors, it might almost as suddenly become dark again. It is far from improbable that this is the explanation of the appearance or disappearance of bright stars, and the strange variations of brilliancy of others which have caused so much astonishment.' This, as aforesaid, was published in 1854. In an

[1] Compare this with Mayer, pp. 560–61.

essay on the 'Mechanical Equivalent of Heat,' published in 1850, Mayer had written thus:—'It is more than probable that the earth has come into existence in some such way [by the collision of gravitating masses] and that, in consequence, our sun, as seen from the fixed stars, exhibited at that epoch a transient burst of light. But what took place in our solar system perhaps millions of years ago still goes on here and there among the fixed stars. The transient appearance of certain stars, which in some cases, like the celebrated star Tycho, have at first an extraordinary degree of brilliance, may be satisfactorily explained by assuming the falling together of previously invisible double stars.'

In placing Mayer and Sir William Thomson thus in apposition I must leave no doubt as to my views and motives. I therefore say emphatically that I believe the essay of Sir William Thomson to be absolutely independent of that of Mayer—that he no more borrowed from Mayer than Mayer did from him. But I desire to show how far a man of natural mathematical genius, nursed in the very lap of mathematics, who, moreover, made the theory of heat a subject of early and special study, had been, as regards the great questions here discussed, anticipated by this obscure Heilbronn physician. The literature of science lay open to its professional cultivators as much as to Mayer, but there was not a man amongst them who, in reference to the forces of inorganic nature, had reached Mayer's level of achievement in 1842; there was not a man amongst them who, in reference to vital dynamics, had reached his intellectual stature in 1845. The same may be affirmed with strict veracity regarding his performance in 1848. Then, as previously, he outstripped all others. Genius was registered in Mayer's organization, and it is only when we compare his opportunities with his achievements—his defective education

with his instinctive and overmastering grasp of phenomena—that the real force of that genius is revealed. It was in the intervals of a laborious profession, and, as he himself informs us, without the slightest stimulus or encouragement from without, that this son of a Heilbronn apothecary so enriched the thought of his age, and built for himself a monument which can never be overthrown.

And what was Mayer's reward in his lifetime? What measure of recognition did he enjoy? The answer is a sad one. Thomas Young had been ridiculed by Brougham in England, and the ridicule was effectual because Young was so far ahead; George Ohm had been pronounced in the Berlin Annual of Scientific Criticism to be the victim of incurable delusion; Mayer in like manner was flouted by his townsmen, and in the 'Allgemeine Zeitung' had been held up to public scorn. As Rümelin sagaciously remarks, that power of concentration which was the basis of his fame, became the source of his misery. He could not shake off the sense of injustice which perpetually haunted him. 'Either,' he said, 'my whole method of thought is anomalous and perverse, and then my proper place would be a madhouse, or I am rewarded with scorn and ridicule for the discovery of important truths.' There was no escape from the one or the other of these conclusions, and the one was as discouraging as the other. He lost his sleep, contracted brain-fever, and in a fit of delirium, on May 28, 1850, he suddenly rose from his bed, and leaped from a window thirty feet high on to the paved street. He was terribly shaken and bruised, but not killed, and before the year was ended he was able to write his profound essay on the 'Mechanical Equivalent of Heat.' But the heart of investigation had been broken within him; he returned to his practice as physician, and though he published some

small papers afterwards, they give little evidence of that marvellous genius which characterised the first eight years of his scientific career.[1]

I met Mayer at Zürich in 1864. His life, he then told me, had taken a new departure. The scorn of his townsmen had passed away, and he was met with respectful recognition where he had previously encountered sneers. He was afterwards ennobled at home, and he received many high marks of distinction from abroad. He died on March 21, 1878. Crowds attended the burial; eloquent and sympathetic discourses were pronounced by eminent men; while the government authorities of Heilbronn drew down their flags as he was lowered into the grave.

The following letter from Dr. Mayer to myself, written at the time when the tide began to turn in his favour, will be read with interest:—

'Esteemed Sir,—I hardly know how to find words to express the feelings which move me at the present moment. On the 16th of last June Professor Clausius conveyed to me the intelligence of your lecture at the Royal Institution. The hopes which in silence I ventured to cherish were more than fulfilled by the recognition which you there accorded me; and I am still more deeply affected by the receipt of your last communications to the "Philosophical Magazine." Your kindness impresses me all the more from the fact of my having, for many years, been forced to habituate myself to a precisely opposite mode of treatment.

'The question of priority as to the mechanical equivalent of heat I regarded as exhausted by my communication

[1] Further reference to the life of Dr. Mayer might here be made. But this I postpone to a future occasion. In his interesting sketch published in the 'Augsburger Zeitung,' Chancellor Rümelin refers to a brief autobiography, written for me by Mayer. This is still in my possession.

to the Academy of Sciences in Paris ('Comptes Rendus,' vol. xxix. p. 534), as my celebrated rival, Mr. Joule, did not, to my knowledge, reply to me. I have referred to this document in a communication to the Academy of Sciences in Vienna (1851, vol. vi., No. 5). Certainly, however, it has never been my desire to diminish in the slightest degree the achievements of the great Manchester physicist. I have never regarded him as an antagonist, but, as you have truly expressed it, have always considered him to be an esteemed and renowned fellow-labourer in the same domain of thought. I gladly acknowledge that, were it not for his excellent experimental investigations, the doctrine of the conservation of force, or, as I should express it, Physical Stoichiometry, would not be able to show the fruits which it now exhibits. The name of Joule, moreover, is quite as famous in Germany as in England.

J. R. MAYER.'

'Heilbronn, May 31, 1863.'

The opinion I ventured to express in 1863, I would here repeat. 'In the firmament of science Mayer and Joule constitute a double star, the light of each being in a certain sense complementary to that of the other.'

NOTE.

THE foregoing sketch had been written and printed before the two most recent German works on the life and labours of Mayer came into my hands.

In these works it is stated that I have suffered myself to be influenced by Professor Helmholtz in a sense unfavourable to Dr. Mayer. To those acquainted with my behaviour in relation to this question such a statement must seem simply

absurd. And with regard to Professor Helmholtz, it is my duty to state that neither directly nor indirectly has he ever sought to bring an influence inimical to Mayer to bear upon me.

Professor Clausius has also been represented as antagonistic to Mayer, but his own reply to this accusation is so conclusive as to render it unnecessary for me to supplement it by a single word.

JOHN TYNDALL.

February, 1880.

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